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PART FIRST.

ORIGINAL COMMUNICATIONS.

ART. I.—*Case of Extensive Osteo-Sarcoma. Read before the Stark County Medical Society*, by L. M. WHITING, M. D., of Canton, Ohio.

The following case is especially interesting by reason of the character and amount of morbid anatomy, presented on examination of the cadavre. But the train of distressing symptoms which attended the decline of the patient, and the mode adopted for the relief of some of the most pressing difficulties, it is thought may also be regarded as worthy of record. Its history, however, is necessarily meagre, for the reason, that like a thousand other individuals in trouble with obscure maladies, he "run the gauntlett" through files of doctors—some of whom may not inaptly be denominated savages, and his sufferings during this ordeal were not a whit behind those of the victims subjected to this aboriginal mode of torture amongst our North American Indians.

The first diseased manifestations were those common to malarious districts. The patient was a man 29 years of age, of nervous temperament, and by trade a Tailor. After being harrassed with intermittent fever, more or less, during a residence of some years on the Ohio canal, in the village of Fulton, in this county, he fell into a condition in which attacks of what are called by "the world's people" *bilious* trouble were frequent, and for the relief of which he took "Anti-bilious pills," and various nostrums—but finally resorted on these occasions to the use of Calomel; the effect of which was temporary freedom, and ability to continue his occupation. In the fall of 1851 he was engaged in an active political campaign, and being a candidate for office, rode much in the heat of the sun during some weeks, and of course was in an excited state most of the time. He now began to have frequent attacks of diarrhea, for the relief of which opiates and astringents were taken repeatedly. Being elected to the office of Clerk in the district court for this county, he removed to this town in February, 1852. He was now enfeebled and pale, and suffered with pain in the rectum, where he discovered some kind of obstruction. After some time spent in tampering with various forms of quackery, for "Piles," he visited Cleveland, and consulted Prof. Delamater, the elder; but (as he said) obtained no satisfactory information in regard to the character of his disease, or relief from his sufferings. Some further time was consumed in fruitless quacking; then he went to Wheeling, where he remained some weeks under the care of Dr. Hullihen, who adds to his brilliant reputation as a dental-surgeon fame in other departments of operative surgery. But he returned from his wandering still the prey of unmitigated disease. The autumn came, and with its falling leaves our poor invalid found himself steadily inclining earthward. His attempts at locomotion were attended with but indifferent success on account of a painful stiffness experienced in the left hip in the locality of which he noticed thickening of the soft-solids. A new prospect of entire relief now appeared in the shape of a burly "*Fellow,*" who assured him that by a purely "Botanic" plan he would very soon be eased of all his troubles. A couple of months proved the pretender to be so near right that it was deemed unnecessary to retain his services longer in order to insure the fulfilment of his prediction.

On the 12th day of January, present year, I was requested to visit the sufferer. Found him barely able to traverse his room with the help of a cane, and complaining of very severe pain in the left leg—in the lower part of the back, and in the bowels from whence it had become a matter of great difficulty to procure any evacuation whatever. In general aspect he was white, haggard and emaciated, yet his appetite was good and his respiration free. On manipulating the lower abdomen and pelvis, the attention was arrested by the presence of a large and very solid growth rising from and filling entirely the left Iliac fossa, and projecting boldly from thence in a round tower shape into the corresponding hypochondrium. Passing the hand over on the dorsum of the left Ilium it was found covered thickly with the same non-elastic tissue. In the right Iliac fossa was another tumor of the same character as that on the left side, but occupying only the pelvis. On the upper, and frontal aspect of the trunk were discovered several of the same degenerations. Four or five, varying from the size of a filbert to that of a pigeon's egg were scattered over the anterior and superior surface of the thorax and neck. On the left temple was situated one of these indurations of the size of a hickory nut, and a much smaller one on the occipital protuberance. A careful examination of the liver detected a feeling of extraordinary hardness in the free border of the large lobe. A digital exploration per anum announced the existence of an induration and thickening in the parietes of the rectum about 1¾ inches from the anus. This seemed to be a complete investiture of the organ, and produced nearly or quite entire occlusion when in a quiescent state. By firm pressure which produced much pain, an opening could be forced through the natural channel about ⅛th of an inch in diameter. This change in the structure of the rectal wall seemed strikingly like the enlargements more exposed and was believed to be the same in character. By some it was pronounced *scirrhus*—but it was *too hard* for scirrhus. The intellect was clear—the tongue clean, and digestion seemed to be performed with integrity. My Prognosis was *death*—and warning was given the patient to look for it under circumstances the most aggravating. The irritation by pressure of the morbid growths upon the large nerves supplying the lower extremity of the left side was already

such as to induce not merely pain, but intense agony; and the constant recurrence of violent spasmodic contraction of the muscular coats of the intestinal canal, by reason of inflation from pent up gases, produced intolerable *colics.* Any attempt to relieve these sufferings by either the forms of opium which are in common use—or which he had tried, was followed by the most prostrating nausea, vomiting, and almost irretrievable constipation. To guard against entire occlusion of the obstructed portion of the alimentary tube, by which he and his advisers were kept in constant terror. His mouth, œsophagus, stomach, small and large intestines were made the constant thoroughfare of all manner of pills, potions, boluses and cathartic compounds; and yet the awful climax seemed constantly approaching. As the hopes of the victim were now fast fading away in all remedial means, and entirely extinguished, so far as boasting charlatanism was concerned, he requested me to take the entire charge of him and make such appliances as I deemed judicious, and most conducive to his comfortable exit from what was to him emphatically a "vale of tears."

For present relief chloroform was prescribed—in liniment over all the painful limb, and inhalation of the pure article to "Point-comfort." For permanent and reliable means of relief, I had—

1st. The substitute for McMunn's elixir of opium, prepared after the formula given in Braithwaite's Ret.

2d. To counteract the constipating effects of this drug, he was directed to take ʒij each day of *ox gall,* and confine himself to such articles of liquid diet as contribute to a soluble state of the bowels.

3d. To be ready in all emergencies for relieving the bowels from undue accumulations, I constructed catheters and bougies, of appropriate size, of *gutta percha,* which were passed through the constricted portion of the canal, the former being made the duct for alvine evacuations. By the adoption of this plan, he was soon brought into a state of comparative comfort. The opiate was taken without regard to quantity, but was graduated by the degree of pain, and the Fel. Bovin. acted like a charm as a solvent of the contents of the alimentary canal. Indeed, it was not long after he commenced the free use of the latter article, before he found himself able to disregard dietetic regulations to a great extent, and eat and drink that

which was most agreeable; and yet by the use of a pump for injecting warm water, followed soon after by a gutta percha conductor, procure daily and copious liquid discharges.

But daily and hourly did the process of morbid change go on in the localities heretofore specified. Steadily the tumefaction increased, especially in the pelvis, the abdomen, and on the temple. At length there came a stage when the remaining soft-solid tissue belonging to the patient, began to disappear with astonishing rapidity, and the fluid secretions of the internal organs to run off without "let or hindrance." Although to all beholders he appeared almost a living skeleton before, he was now seen manifestly to *grow poor* from hour to hour. In a few days a shadow was all that was left of our friend, and he succumbed to the great destroyer, with his mental eyes wide open, but a striking instance of human attenuation. His death occurred on the last day of April, late in the afternoon, and a post obit. examination was made at ten o'clock the following morning, in presence of Drs. Hurxthal and Whiting, of Massillon, Dr. Slusser, of Fulton, and Dr. McAbee and myself, of this town. The following notes were made of the autopsy.

External appearance, that of a skeleton, with integument drawn tightly over each individual bone.

Tumor in left temporal region first dissected out. It was found firmly adherent, apparently growing upon the bones of the skull, which it covered. The periosteum was entirely gone underneath the mass, yet the structure of the bones was not materially changed. There was, however, slight softening of the external table of the os frontis. On an incision being made through the integument the growth appeared enclosed in a non-adherent envelope, which appeared to be the temporal fascia. In its longest axis (antero-posterior) the tumor measured two and a half inches, vertically two inches, and had an average thickness of one inch. Structure, *osseous* and *spiculated*; the spiculæ standing at nearly right angles with the plane of the bones from whence they sprung. The abdominal and pelvic cavities being exposed, the most prominent feature which presented itself, was a large body with a smooth and shining surface, filling the left half of the upper portion of the cavity of the pelvis and rising boldly into a great promontory in the hypochondrium of that

side. This was exceedingly indurated, and on attempting its removal was found to be connected with the entire inner surface of the left illium in precisely the same manner as the tumor on the head was with the bones of the skull. Its structure was osseous, and spiculated like the former. Its color yellowish white. This pathological specimen measured before removal six and a half inches in its vertical axis, and three and a half antero-posteriorly. In the right illiac fossa was another growth of precisely similar pathological character, and attached to the bone of that side in the same way as the two already described. This measured three inches in its vertical axis, and one and a half in average thickness. Scattered along over the frontal aspect of the last two lumbar and sacral vertebræ were indurated growths bearing the same general character as the larger ones, and varying in size from that of a pea to a hickory nut. They were osseous. The presence of so large a quantity of foreign material in the cavity of the pelvis, of course reduced its capacity to retain in a natural position its appropriate contents, and much suffering was the result. The bladder was covered on its exterior surface with reddish colored indurations, varying from the size of a small shot to that of a pea, very hard, but not yet bony.

The rectum, although firmly secured by adhesions, was carefully taken out of its place, and found to be thickened throughout its entire extent. At the distance of one inch and three fourths from the anus it was found invested in its entire circuit with a very hard deposit three eights of an inch in thickness, and extending from the point already designated, one inch and a half upward in the long axis of the tube. This we believe to be *osteo-sarcoma*, for although the osseous structure was not so manifest as in the larger tumors, yet on cutting through the diseased mass the cracking sound as of fracture in minute spiculæ of bone was distinctly heard, moreover crepitus was also perceived on pressure between the fingers, and the roughness of incipient ossification could be felt.

The liver was studded over most of its convex surface, and much on its opposite side, with degenerations. These were lenticular in form, and varied in size, from that of a split pea, to a large sized seed of the strychnos nux vomica, which last body in many respects they strikingly resembled. All these degenerations had rais-

ed margins and depressed centres, forming little basins. The impression created by a view of them was, that a purulent fluid had existed underneath the envelope of the liver; that this had been nearly absorbed, and the covering raised by the presence of this fluid, had fallen back into the pit which the absorption had created. But all were alike solid, and all parts of each individual degeneration were alike solid, and all approximated bony structure so clearly, as to be pronounced the same in pathological character with those portions of the decidedly osseous tumors which had not yet become entirely converted into that tissue. The free border of the the large lobe of this organ was entirely changed. Color, pearly; texture, apparently cartilaginous; but upon handling and cutting we discovered the same impassibility and crepitous, (the latter very slight) which characterized all the other morbid specimens not *decidedly* osseous. The gall bladder was much thickened, and the walls *solid*. This morbid character of the structure of the liver extended upward upon and through it to the height of three inches, and in some points nearly four.

The diaphragm was covered upon its inferior surface with the same reddish colored elvations as those found distributed over the urinary bladder. In the pancreas was found an unequivocal osseous formation in size equal to a small garden bean, and similar in form. Adhesions abounded throughout the entire abdomen and pelvis, which of course rendered the dissection tedious, and a perfect exposure of the pathological anatomy difficult.

I would, however, take occasion in this place to express my sense of obligation to my yonng friend, Dr. H. M. McAbee, who kindly took upon himself most of the labor, and who so performed it, that we have been enabled to preserve a uumber of very valuable pathological specimens, which I have the pleasure to present for your inspection. The induration and enlargement on the dorsum of the left illium was not removed: but a dissection was made over the crest of that bone, which exposed a fan-shaped expansion of the diseased tissue connecting the mass within the cavity with that upon the external surface of its parietes. This external tumor was large, and had all the characteristics of that described as springing from the opposite side of the same bone, and no doubt was *osseous*. The same

was undoubtedly true of those located on the superior and anterior portion of the thorax and neck, as also that upon the occiput.

To sum up this already protracted and somewhat verbose report, permit me to remark, that I regard the case as a very remarkable one. I know of none on record in which such a vast amount of osteo-sarcoma is described as existing at all in a single individual; then the extreme rapidity with which the large masses of this material increased, is matter of surprise to me. Other items of interest will suggest themselves to your minds.

I close this paper with the enquiry, whether with a full knowledge of this proclivity to morbid osseous development in the human body' it might not be made to cease, if in its early stages this perverse disposition was subjected to such remedial means as would be the result of a thorough knowledge of the *chemistry of man.*

ART. II.—*Communicated,* By L. M. WHITING, M. D.

SUDDEN DEATH IN THE PUERPERAL STATE.

CANTON, July 28th, 1853.

Dr. HOWARD—*Dear Sir:* Some days since, as I am informed, the Secretary of the "Stark County Medical Society" forwarded to you a report of a case possessing some interest, and which was communicated by myself at the last meeting of that body. Associated with that case was another, which will doubtless be regarded by yourself of some interest, and to those who feel any special interest in the investigations with reference to *sudden death in the puerperal state,* it may be considered of sufficient importance to "make a note of."

Mrs. S——, wife of the patient who was the subject of the foregoing report, was a short, round, compactly formed woman, of twenty-eight years; had been married about six years, and was the mother of two healthy daughters—one four and the other two years of age. When I first saw her—about the first of January last—she was advanced to the sixth month of pregnancy, and then engaged in hourly attendance upon her husband, by day and night. By this time his case had reached a degree of malignancy which rendered many offices of kindness very offensive to the attendant. Yet the duties of nurse were assumed and performed by her with the great-

est cheerfulness, and in spite of all the remonstrances her friends and himself could interpose. This course was pursued during the entire remaining period of gestation—the circumstances constantly growing more and more unfavorable for her well-being At length the time of her accouchment arrived, and I was much gratified to find her lying in a room separated, as effectually as could be, (on the same floor) from the rapidly declining sufferer who had so long engrossed her entire thoughts and personal care. Her labor was somewhat severe, though not tedious, and concluded without any untoward occurrence. The next day she was very comfortable—but on my visit, judge of my surprise to find her husband sitting, bolstered up in a large arm-chair, in her bed-room. This I regretted exceedingly, but as the circumstances were peculiar I did not materially complain—supposing of course, that he would soon become fatigued, and be carried back to his own apartment, in a remote part of the house. But if I was surprised in the morning, I was thoroughly astonished in the evening, by finding him fairly domiciled in a large parlor communicating directly with the dormitory in which lay his wife. I immediately expressed my fears to her friends of this proximity, but she soon learned, by some means, that his removal was contemplated, and interfered with a most positive prohibition. In consequence, he was permitted to remain. On the third day after her confinement, she had the slightest possible chill, followed by pain in the uterus, and fever, both of which, in a few hours became intense, the former radiating over the most of the peritoneal surface, which was intolerant of the slightest pressure. Being on the lookout for trouble of this sort, we were prompt in the adoption of remedial means, and had the satisfaction to see them successful. In a very few days there seemed to be an entire extinction of the puerperal fever, and all interested felt free of apprehension. She was well. On the ninth day she arose, walked into her husband's room, remained some time, held her child in her arms while there, a portion of the time, and finally walked back to her own apartment. This was, however, a day of great trial to her feelings—her husband had, during her confinement, failed very fast, and the change startled her into the conviction that he must die very soon, and she wept a great deal, yet rested during the night, and in the morning seemed more

duties of nurse were assumed and performed by her with the great-

cheerful and better than at any time previous. Her brother, (Samuel Krider, Esq., of the last Legislature,) went into her room and received the assurance from her that she felt very well indeed, and that she intended to sit up nearly or quite all day. I saw her and husband early in the morning, but supposing her to be asleep, and having an urgent obstetrical call four miles in the country, I did not arouse her. About nine o'clock A. M., her mother went to assist her in getting up. She handed her a *pot-de-chambre*, which she expected to use in bed for urinary evacuation. Just then Mr. S. made a sound of distress, when she said to her mother—"Go out to him—he wants something; I can help myself perfectly well." The old lady did as requested—left the room, and as she went out closed the door. But a moment had elapsed, when those in the adjoining room heard a heavy fall, and running instantly, found Mrs. S. lying on the floor, the bed clothing still enveloping the lower extremities, and apparently *dead!* She was immediately raised into bed, and one of her friends in attendance, supposing her to be in a fainting state, asked her if she would have water, to which she replied no, then breathed once or twice, and stopped forever! One or two physicians of the town were called in as soon as circumstances would permit, but nothing essential was done, each regarding death as certain.—Being absent, in attendance on the case as above stated, I did not learn the facts until two or three hours had elapsed; when I reached the scene of disaster, of course all was "stark and cold." I learned that the urinal had not been used, and could find no signs of injury whatever, from the fall. The peculiar circumstances of the family rendered a post mortem examination impracticable. What was the cause of death I do not know. There had been no drain of the blood-vessels to induce a belief in *heart-clot*, and it would seem fanciful to refer the fatality to the introduction of air into the uterine sinuses. It was a terrible blow to the already dying man, whose hopes for the well-being of his little ones were all centred in the assurance that their mother was young—remarkably healthy and full of the milk of human kindness and maternal love. He lingered yet four days, and then joined his most faithful and cherished friend in that land where "there shall be no more death, neither sorrow nor crying."

By the above statement you perceive I was engaged in a case of midwifery in the country, at the time the sad catastrophe occurred.

This woman "did run well" for three days; she was then seized in the latter part of the night with violent pain in the left lower extremity. In the morning her husband came to town to inquire what should be done for the pain in the limb—remarking at the time of consultation that she had some two or three months previous suffered severely with a precisely similar pain in the arm and shoulder, but on using some stimulating embrocation it disappeared entirely. He was directed to return immediately, and administer a free dose of morphine. Before he arrived at home, however, he was met by a messenger sent post haste to inform him and myself that the pain had shifted its locality, and was now in the chest, and as she thought, threatened her with immediate death. The messenger came on for me, and the man went home and gave the morphine. When I arrived I found her very comfortable—not much pain anywhere. The abdomen and pelvis was carefully manipulated—no sign of trouble there; no fever; lochia not abundant, but steady. I regarded the suffering as neuralgic decidedly. There was a *little* secretion of milk. Previous to the labor, she had, during the night, a diarrhœa, but nothing like it after. At this visit I ascertained that she had been impressed with the idea that she would never recover from this trial. She was a woman of decidedly nervous temperament. Had borne three children—the youngest seven years old. Cathartics, when necessary, operated kindly. Sometimes there was excessive tenderness on pressure, and pain, on motion, in the lower abdomen —at others there was none; opiates always controled it when it existed. But she gradually declined, and died in twelve days. Being satisfied she would die, she refused to take such medicines as I thought calculated to sustain and save her, and I quit her entirely two or three days before her death; before doing so however, I called other medical counsel, who declared they could see no good reason why she should not recover. What destroyed her I do not know, it is proper, however, to remark, that about this time I had several other patients who suffered, in child-bed, threatening symptoms; there were several cases of erysipelas scattered about, and two or three weeks previous to the occurrence of this case, I was called in counsel over a case of unequivocal puerperal peritonitis, in its hopeless stage, only two or three miles from the last.

Yours truly,

Art. III.—CONTAGION.

Proof-Essay from "General Pathology." By Dr. A. de Leczynski, Columbus, Ohio. (Now in progress.)

When we hear the name of a fellow-being the question naturally suggests itself—"*who* and *what* is he?" There are many subjects, although known as long as human record reaches, which have not been defined, and at present are not known better than centuries ago. And *why* is it thus? *Because human infallibility and wisdom have decreed it so.* In place of becoming the scholars of nature and the investigators of her actions, men had the insolence to *regulate* her movements by hypothetical laws, *invented* upon the strength of *superficial observations*; and herein none has been stronger than our —the *medical profession.* From its divine origin it sunk until, at a time, it was hardly more than *privileged charlatanry.* But, thank God, a brighter morn is dawning! And what has been the result—have we progressed and gained in experience? No—far from it—with unlogical reasonings and deductions, the consequence of mal-perceived and mal-construed observations of Nature's actions, we became the losers, morally and physically. A good instance is given by the subject "contagion." There are many hypotheses as regards its nature, but schools and scholarships who enriched us with the former, have still given no clear definition of it. There are parties who deny the *contagiosity* of certain diseases, which they estimate to be only *infectious;* there are yet other parties who believe certain diseases to be neither *contagious* nor *infectious,* but *portable,* —but what they understand by *contagion, infection,* or *portability,* they have not had the goodness to inform us of.

Let us review the words etymologically:—*Contagium* is formed of *con* (cum) and *tango.* The former word signifies *with, through,* whilst the latter means, 'I touch.' In the sense the medical profession uses it, it conveys "*the introduction of an infection into an organised (not also unorganised!) body, by means of bodily, therefore actual contact.*" *Any disease capable of reproduction, by transmission of its germ, we call logically, "contagious."*

Infection is likewise formed of two words—of *in* and *facio.* The former means, *in, into;* the latter *I make, I do, I cause.* We under-

stand by it the introduction of a mass—of an agent, into the human body exclusively. The ancients understood by it not only *this*, but also the introduction of other matters into other bodies—as for instance, the introduction of colors; wherefore, by *infection* they also understood the *process of dying*. Literally, contagion and infection mean the same thing, pointing only etymologically as to the *manner* in which this takes place. So some schools have understood it when they say, the former, (contagion) can only be communicated by *actual, immediate contact*, from man to man; whilst the latter, (infection) will be communicated by *mediate contact*. Some schools reverse the definitions, in opposition with the etymological import of their names.

Portable comes from *portāre*—'to carry.' Logic teaches when a portable matter causes a specific disease, *it must be a contagion*.

I believe there was no necessity for these subdivisions, and all we gained for our labor has been *confusion*. From the preceding we conclude that contagion and infection imply the same matter—contagion, and the only difference exists in the one being the name for *general* whilst the other is the name for *fixed* contagion.

We understand by contagion a noxious principle, which, engendered in the animal or human organism, and transferred to another, or others of the same or higher species, will cause, always, *the same disease*, of which, primarily, it has been the product. We shall endeavor to delineate its *genesis*. When atmospheric and telluric noxious influences produce upon the organism so great a chemical change as to cause a disturbance of functions of the human fabric, then we call this abnormal state disease. The abnormal, like the normal state, has its products and educts. When the functions are changed, the organic structures must have been changed, wherefore an alteration of the products and educts of the body. The more intense the organic and functional changes of the body are, the greater will be the abnormal state of the products.

When during the prevalence of endemical or epidemical disease, many persons, attacked by it are enclosed in relatively small rooms, to which not enough fresh atmospherical air can be admitted, then these rooms will not be filled with carbonic acid, but with animal products of exhalation, and perhaps those of the voided excretions, which now will putrify, and not only produce the original abnormal process,

but also a far more virulent one, causing either dyscrasies of the blood, and thereby an affection of the nervous centres, or they affect the latter primarily and the blood secondarily. The new product we call *contagion*, the manner in which it orignated its *genesis*.

It is not requisite that contagion be generated only in enclosed rooms. If many individuals, under similar conditions, are attacked in a low malarial region, where the change of atmosphere, on account of its geographical position, is impossible, or difficult, (as in valleys to which the winds have no access,) and where perhaps yet planetary influences lend their coöperation, (as the hot rays of the sun, promoting rapid decomposition,) then contagion will likewise be generated, and will be more lasting, as it finds a ready nucleus in malaria—of this anon. This I conceive to be the process in which contagion of wide spread epidemics is produced. In this manner the following and other sporadic and endemic diseases change into contagious epidemics—yelow fever, scarlatina, variola, the plague, tussis convulsiva, cholera Asiatica.

A contagion, once generated, on account of its more homogeneous nature, acts entirely different from the mother-disease, and when carried to other places, appears there as contagion, provided certain circumstances exist there favorable to its existence. It passes not through the same process described before, nor will it need those peculiar favorable conditions as were required for its genesis. It appears as contagion and will exist as such until excluded by other causes.

But it is by no means only the animal organism which contagion may affect. When introduced into a new place, it affects the soil. This is proved by contagion becoming an endemic disease in a place where first it was introduced as a contagious epidemic. (The revelation of these laws belongs to the province of Natural Philosophy.) In this manner, the yellow fever, a native of the West Indies, has become a native of some parts of Spain. It is not generated by the growth of American maritime plants, in places where now it is an endemic, as medical philosophers tell us. If so how does it happen that *variola*, a native of Europe, has become a citizen of this continent? Are there also certain plants, on whose presence the existence of the latter disease depends?

In order that a contagion may be received and regenerated, the soil must be as much predisposed to it as the animal organism. In this manner we can understand why some places are exempt from an epidemic, whilst others are visited by it terribly, and why an epidemic likes to linger in some places longer than in others. The most fruitful hot-beds for contagion are malarious districts—malaria formtng the nucleus to the introduced, and the associate of the new forming germ. In order to understand better the relationship between contagion and malaria, we shall speak about the genesis of the latter.

Those districts of the earth where a luxurious vegetation exists, and where, on account of their position and different influences, decomposition is carried on more rapidly, are generally situated low, therefore well watered. They mostly lay in situations where heat and sunlight support rapid oxydation—vulgo, decomposition. The product of this rapid oxydation are exhalations of the oxydizing bodies—in other words, minute particles engaged in a transitory process, which, on account of their lightness, will be mixed and diffused in the air. Simultaneously with these exhalations, vapors arise, into which, by the aforementioned causes, water has been reduced. On account of their weight, these exhalations cannot rise above a certain height, and here, with the proper quantum of moisture, under the influence of the rays of the sun, they decompose still further and more rapidly. At night, on account of their gravity, they fall—with their solvent, in form of fog, supported only by an under-current of air—so low as to become hurtful to animal and other organisms. If the latter can not assimilate these matters, disease will be the result. These matters we call *Malaria*. This word is Italian, and signifies *bad air*. Malaria and Miasma intend to express the same. Miasma means defilement, contamination, from *miaño* to color to defile, to contaminate, and is related with *mingo* to urinate.

When malaria and the products of animal organism unite, then the new product receives power to infect the soil, in virtue of its *planetary* origin, and the animal organism in virtue of its *animalic* descent. These new products are contagia.

Contagion we divide into *general* and *fixed*. To the former belongs

the virus of contagious epidemics; to the latter those whose origin is spontaneous, autochton, as syphilis, scabies. We discriminate in contagia—

1. The body, the basis;

2. The animating principle;

The body of the contagion may be either,

a. Drop-like fluid—as in syphilis, the chancre-matter; in hydrophobia, the saliva;

b. In the form of vapor—as in yellow fever; putrid dysentery;

c. Or gaseous—As the contagion of scarlatina; or

d. Appearing in all these forms united; (variola.)

The basis of contagion appears to act always alike; but its nature, in different places, climates, and at different times, is subjected to certain changes. An instance of this is afforded by syphilis, the contagion of which, according to old authors, appeared at its first *debût* as a gas. After it came gonorrhœa, as a new character.

Of the animating principle little is known, but it seems related to positive electricity. This doctrine seems to be sustained

1. By the fact that it is always perceived by the senses of smell and taste, which is also the case by galvanism. If we introduce two pieces of different metal into the mouth, one above, the other below the tongue, and bring them into contact with each other, we perceive a salty taste.

2d. By the fact that ideo-electrical bodies are the best conductors of contagion—as glass, silk, wool, resin. The conductor or medium of that contagion, the basis of which appears in a vaporous or gaseous form, *is the air*—as in variola, the plague, cholera. The conductors or medium of that kind of contagion, the basis of which appears as a drop-like fluid, are, without exception, compact bodies, as silk, wool, etc. But compact bodies can also transport the former form of contagion.

The capacity of the air, as a medium, seems to be increased or decreased according to the difference of temperature. In conformity with this law, the *plague* will discontinue in a very high degree of heat.

In order that a contagion can be reproduced by the organism, it must, by its mediums, be deposited upon mucous membranes

—natural or artificial ones. Chancre-matter, when brought in contact with the epidermis, will cause it, but reproduce itself if the epidermis is removed, or when brought in contact with mucous membranes. Contagia cannot reproduce themselves before they have arrived at maturity, some only at the state of their deflorescence, as typhus, variola. Some of the contagia will attack the organism only once, whilst others, having invaded it once, increase thereby the receptivity for it, as we observe in syphilis.

There seems to exist a hostile disposition between contagia—that is, they exclude each other. According to this law vaccine protects against variola; measles against tussis convulsiva—some think vaccina very protective against the latter disease.

There is one thing yet I should like to speak of. I know I am treading upon forbidden ground, following examples I must condemn —I mean *hypothetical* reasoning—but the hypothesis I wish to forward is based upon facts. Should I be *wrong* I beg to be forgiven; if *right* perhaps others will investigate further, and develope a *law* from the present hypothesis.

While perusing Humboldt's "Cosmos," I met with the chapter on animalcula and infusoria. When we reflect upon the enormous quantity of these microscopical beings, the question forces itself upon us—"can these beings have any bearing in the production and transmission of diseases and contagions?" I should answer, "I firmly believe so." Humboldt says: "Although the surface of the ocean is less rich in living forms than that of continents, the oceanic depths exhibit to the astonished sense (certainly per microscope) a multiplicity of animalic life." Again, "the abundance of these maritime animalcules, and the animal matter yielded by their rapid decomposition, are so vast, that the sea-water itself becomes a nutritive fluid to many of the larger animals."

Ehrenberg investigated, and Humboldt endorses the following: "in the animal kingdom those forms which we term microscopic occupy the largest space, in consequence of their rapid propagation. The minutest of the infusoria—the monodidæ—have a diameter which does not exceed $\frac{1}{3000}$th of a line, and yet these silicious-shelled organisms form, in humid districts, subterranean strata of many fathoms in depth. Although the existence of meteoric infusoria is

more than doubtful, it cannot be denied that in the same manner as the pollen of the flowers of the pine is observed every year to fall from the atmosphere, animate infusorial animalcule may likewise be retained for a time in the strata of the air, after having passively been borne up by currents of aqueous vapor."

Again, de Barre, Beckwith and others, melted flint, and immediately as it cooled, before any insect had time to deposit its eggs within, they let a galvanic stream into the inanimate dust, calling by it myriads of animalcules into life, which afterwards propagated themselves. Were they produced by spontaneous generation? By no means! (Compare Ehrenberg on the revival of animalculæ that had been dried during a space of many years.)

My deductions from the said are as follows:

1. As in the animal kingdom those forms which we term microscopic occupy the largest space, they form also the largest aggregate of matter.

2. Whenever there occurs, from one cause or other, an unusual mortality among the microscopic beings, there must necessarily be a reäction upon the state of health of the larger animals, in consequence of the decomposition of these infusory bodies.

3. The unusual large decomposition of animalcules, in connection with exhalations of decomposing vegetable matter, are the source of such diseases of the blood and nerves as appear in general contagia, engendering in the human body changes similar to their own.

4. That when these contagia are carried by the air, and other conductors to places not yet infected, and where animalcules are very numerous, it will either first affect the latter, and secondarily mankind or *vice versa*.

Gardiner, in his Medical Chemistry, when speaking of malaria, says: "I do not however agree with Prof. Daniell, and others, that sulphureted hydrogen is the malarious body, but believe that sulphur is a component of a certain unstable organic body, which constitutes the poison." This organic body may be obtained by precipitating the vapor from a marsh, in the morning before sunrise, and it blackens sulphuric acid. It consists of sulphur, carbon, hydrogen, and probably oxygen. (Is there no nitrogen in it?) It readily decomposes, emitting a *putrid* odor, (and contains no nitrogen!!) and appears to

be decomposed by the rays of the sun. It is destroyed by fire—does not rise high in the air—is decomposed by plants which appear to remove its water, an essential ingredient of the body. My opinion is that the malarious body is animalcules in the putrid state. In favor of this stands the *putrid odor* Gardiner speaks of.

Some may deny that diseases of the lower animals can be transmitted to man, or vice versa. I answer that at a time when stomatyphus (Stomacacé Fegar) was endemical in certain places, I have seen *dogs* and *cats* affected by the same disease. Here we see the same cause produce simultaneously the same results in man and the lower animals. I have noticed further that saliva of a man laboring under hydrophobia, introduced into thec irculation of a dog, can cause the same disease in the latter.

This proves that certain diseases of animals, transmittible to man, are retransmittible from man to animals. From this we deduct that pathological processes of animalcules transmitted to man, can be retransmitted from man to animalcules.

As another proof that an abnormal mortality of infusoria can be the cause of frightful mortality of man, I am reminded of the occasional contact of the sea with sweet water. It has been generally taught that the dreadful malaria produced by this union is caused by the action of the salt water upon the vegetation of fresh water marshes. Experiments deny this. Malaria is caused here by the great mortality and decay of both the infusoria of the salt and the fresh water. Neither can live in the strange element. A similar observation can be made by trasnmitting small fish from sea-water into sweet water, and vice versa. Neither will live long in the strange element. Upon this law I am relying when giving the above explanation.

Lastly, I beg not to be misunderstood. I do not intend to say that contagion is the product of putrescence of an abnormal number of infusoria. I only say that this can be the cause of endemics or epidemics, and that, in order contagion may be formed, those circumstances must take place enumerated when giving the definition of contagion.

ART. IV.—*A newly discovered principle in the Urine.* (From the German.) By Dr. A. de LEZCZYNSKI.

Stædeler speaks, (in Wœhler's and Liebig's Annal, vol. 11, and Frorieps' Tagesb. No 308,) of a newly discovered principle in the

urine, which he names *phenylic* or *karbolic acid*, which, he tells us acts as a violent poison upon the organism of man and animals.

The *hydrated karbolic acid* is a colorless, oleaginous fluid, its smell resembling castor—its taste is burning. When brought in contact with the skin it causes the latter to corrugate to a degree resembling deadness. Animals to whom it has been administered internally, become very restless, and die convulsive.

Brought into contact with blood and albumen it causes them to coagulate. It is powerfully antiseptic and removes from putrifying matters and excrements their smell. The manner in which Stædeler procured this principle is as follows: he mixed recently voided urine of cattle with hydrate of lime, and added hydrochloric acid, after which he subjected the whole to distillation. The latter process gave a milky fluid of an extremely unpleasant odor, from which he separated a few drops of oil of a tenacious nature, and yellowish, or yellowish-greenish color. By repeated rectification he succeeded in condensing the volatile matters still held in solution by the water, and the result was an oleaginous yellowish fluid, which sank in the water passed by distillation, and which, for the greatest part, consisted of *karbolic acid*.

Another principle related and frequently united to this acid, and which is highly carbonized, he names *taurylic* acid. They are distinguished from each other by the higher boiling point of karbolic acid.

This experiment has proven besides, the existence of *two other volatile acids*, in the urine, which Stædeler calls *damaluric* and *damolic acids*, but as they are found likewise in the perspiration and the fatty acids, they cannot be said to be peculiar to urine.

COLUMBUS, O., July 1853.

ART. V.—*Note-Book Gleanings.* By SAMUEL C. MENDENHALL, M. D., Bloomfield, Warren co., O.

PLACENTA PREVIA—VIOLENT UTERINE HÆMORRHAGE—PERFORATION OF THE PLACENTA BY THE CHILD'S HEAD.

Aug. 20th, 1852. Was summoned in haste to see Mrs. C., aged 28; residence, Hilliar township, Knox co. Found that while atten-

ding to her domestic duties, uterine hemorrhage of the most alarming character had occurred. Some three hours had elapsed from the occurrence of the attack before I reached her bedside. The bed and bedding were soaked with blood; pulse at the wrist scarcely perceptible; countenance exsanguine; prostration extreme; uterus contracting feebly and irregularly. Considering the danger eminent, I immediately adopted the most energetic means for arresting the hemorrhage, regardless, for the time being, of every other consideration. Ligatures were drawn tightly around the upper and lower extremities, close to the axilla and groin; cold water perseveringly applied over the hypogastrium, and *large* doses of acetate of lead were administered, with a little morphine occasionally. The hemorrhage began presently to subside, and in twelve or fourteen hours ceased. She was in the fifth month of her pregnancy, and had suffered from severe flooding at both her previous accouchments, but never before during pregnancy. Rest, in a horizontal position, for eight or ten days, and a careful avoidance of severe exercise, were enjoined, and I heard no more of her until

Thursday, Jan. 13, 1853. I was again summoned, and found that she had been in the care of two midwives, since the preceding Monday. Irregular pains, of an ineffectual nature, had harrassed her during the whole time, until Wednesday evening, when they ceased entirely. Her strength was nearly exhausted; the parts, on examination were found relaxed, the placenta presenting at the mouth of the uterus. There seemed to be a necessity for prompt delivery; ergot, in full doses, was administered; the uterus began to contract pretty strongly, and about 8 o'clock, P. M., she was delivered of a female child weighing nearly eleven pounds—apparently lifeless, but by the use of the warm bath and artificial respiration, animation was restored, and the child is now (Aug. 6,) living.

Upon attempting to remove the placenta, it was found adherent, and on introducing my hand into the uterus, to my astonishment I found it perforated by the passage of the child, it being attached a little to one side of the os uteri, and so firmly adherent that it was with the utmost difficulty that I succeeded in peeling it off from the maternal surface; it seemed indeed to be almost continuous in structure with the substance of the uterus. Her recovery was tardy, and

the small portions of the placenta that were not removed, caused considerable annoyance, from the fœtor they imparted to the lochia.

The hemorrhage, in the first instance, was doubtless caused by the development of the gravid uterus separating, in part, the placental attachment—the subsequent unusually firm adhesion consequent on such separation. The rupture of the placenta was produced by throes such as are seldom witnessed. It would have been better, in my opinion for her to have been delivered when the hemorrhage first occurred. What say the "*Dii majores professionis*" on preventing abortion in case of uterine hemorrhage depending on placenta previa?

DYSENTERY.

August 21st. 1852. I was called to visit Mr. J., aged 48; nervo-bilious temperament; rather a feeble constitution; residence near Sunbury, Delaware co. Found him laboring under a severe attack of dysentery, popularly called "flux," then prevailing epidemically in that region; pulse 115, small and quick; countenance anxious; eyes sunken; dejections, dark, grumous, and mixed with the *debris* of broken down tissue, horribly fœtid, and occurring six or eight times per hour. Had been under the care of some of the "Botanic crew," until "composition," "lobelia," &c., failing to prevent the disease from progressing from bad to worse, his friends insisted upon a change of treatment, and I was sent for. One of the misnamed "doctors" was by his bedside when I arrived. As I took no notice of him whatever, proceeding immediately to examine and prescribe, he seized his medicine case, and with imprecations dire on the "calomel doctor," left the premises.

Ordered him to take a powder composed of half a grain of morphia, two grains of ipecac, and three grains of tannin, every six hours. A thorough application of a saturated solution of camphor in spts. of turpentine, over the stomach and bowels, every two hours; an injection of kreosote suspended in water, three drops to the ounce, to be given once in four hours, until the fœtor of the discharges was corrected, and then to be replaced by injections of acetate of lead suspended in mutton broth. No cold water allowed—mutton broth being the only drink permitted.

Under the above plan of treatment, (modified, of course according to circumstances, as the disease began to give way) he gradu-

ally recovered, so as to be able in six or eight days to sit up in a chair, and in about three weeks resumed his usual employment.

Dysentery, as it prevailed in this portion of the State in the summer and autumn of 1852, was far more difficult to control than the same disease the preceding year. There was an unusual tendency to "putrefaction of the fluids of the body," if I may be allowed the use of such a phrase. The course of the disease was violent, rapid, and in many instances fatal, especially when occuring after measles, which visited us epidemically, early in the season. I cannot, nor can any of my brethren in this region, boast in the language of one of your contributors, that "none of our patients have bit the dust." In many very unpromising cases, however, I am confident that a system of treatment like that given above, has saved life. Opium, kreosote, turpentine, camphor and acetate of lead, judiciously used and combined with proper adjuvants, will do much to control this fearful scourge, yet the experience of those of my brethren who battled with this agonizing disease in 1852, will bear out the assertion that too frequently our best efforts were unavailing, and that the epidemic was characterised by a malignancy and obstinacy seldom witnessed.

ART. VI.—ON THE ANALYSIS OF THE BLOOD. *An extemporaneous Discourse; delivered before the Cincinnati Medical Society.* By JOHN LOCKE, M. D., President of the Society.

GENTLEMEN OF THE SOCIETY: Without attempting to enter into the subject of the physiology or pathology of the blood, I shall merely undertake to give you a plain account of the analysis of that fluid, by Dumas, as modified by Andral.

The manipulations will be such as have succeeded in my own hands. The analysis as given by Andral does not profess to be an ultimate one—for it neither gives the amount of carbon, hydrogen, oxygen or nitrogen; nor does it give the various salts, the iron, the phosphorus, or the sulphur. It merely proposes a very simple separation, which shall answer mostly pathological purposes; it proposes to show the quantity of the fibrine, the serum, the globules, and the water. The process is made so simple by the distinguished men who have devised it, that I have recommended it as a first encouraging process to new beginners in organic chemistry.

The principles on which it depends are two—1st. That you have given the whole of a compound, containing two elements, and can, by analizing a part of it, obtain the ratio or proportion which they bear to each other; then, by calculation, you can determine the separate quantities in the whole mass; 2d. That if you have a compound substance, given in weight, and can, by analysis, determine all except one of the ingredients, that one can be obtained by subtracting the sum of all the others from it, or algebraically—Let a = the whole quantity, (known of course) and let that be composed of the unknown quantities x, y, and z—viz: $a = x \times y \times z$; now if by any means you can determine the value of x and also the value of y, then $a - (x \times y) = z$, and z becomes known. But this principle is so simple that "making x's about it" only makes it more obscure. If a market woman has a mixture of peas, beans, and corn, the whole weighing 10lbs., and should find out that the peas and beans weighed together 7lbs., she would not be long in finding out how many pounds of corn there were. But to the analysis:

The blood for the purpose need not be drawn in any peculiar manner, though it ought to be left at rest until it coagulates. Then will be found floating a transparent, though not colorless serum, upon the top; this serum is to be carefully decanted off, and if, from any mismanagement, a little portion of red globules are still intermingled, which often happens, it must be left until these entirely subside, which will ultimately take place. The transparent serum is then again decanted, and the residue returned to the clot. It must be recollected that this separated serum is not an attempt to get all the serum from the blood, for there will still be some diffused through the clot. The effort is only to attain a sufficient portion of it to operate upon. The amount of the remainder is to be obtained in a manner presently to be described. This separated serum is to be accurately weighed, as well as the whole amount of blood before the serum was removed, and by subtraction of the weight of the separated serum, there will be left the weight of the clot and whatever it may contain, which must be noted. The next step is to place the partial separated serum over the water-bath for evaporation and desiccation; this operation is intended to analize this portion of serum into its water and solid serum—we shall call this *partial analysis.*

The nurse-lamp water bath, described in my paper on the urine, and published in the Lancet for April, 1853, will answer the purpose well, the wood being placed in one of the saucer-shaped tin pans there described.

Fibrine.—While the partial serum is evaporating, the operator may engage in separating the fibrine. This may be done in any way by which the blood globules can be washed away by means of water. At first, water may be poured into the vessel containing the clot, and be poured off again, carrying the globules along with it. All these washings must be saved. But finally I have found it best to envelope the clot in a rather coarse cotton or linen cloth, and tie it up tightly; then kneading it with the fingers, or washing it with water gradually separate or wash out the globules. When the globules cease to escape abundantly, I have found it to facilitate the process to untie the cloth occasionally and tear the condensing fibrine, thus presenting the interior and more bloody portion next to the cloth, when again, on tying up and washing, abundance of globules will escape. By a good deal of time and patience, the fibrine will be washed to whiteness. It is then to be cleanly removed from the cloth, and dried over the water-bath. It is finally to be completely desiccated by being placed under the exhausted receiver of an air pump, along with a separate vessel of sulphuric acid, and the pump occasionally worked until the fibrine ceases to lose weight. The fibrine thus procured must be immediately weighed and the weight noted.

This will be the entire weight of the FIBRINE belonging to the quantity of blood analyzed.

Partial portion of the Serum.—When this shall have been dried in the same manner as the fibrine, it is to be weighed, and the weight noted as a *part* of the serum; the loss of weight in drying should also be noted as a *part* of the water which the blood contained. The ratio or proportion of the solid dry serum to the water is also obtained and may be expressed as a fraction by placing the weight of the dry serum over the weight of the water as 10-100.

We finally proceed to evaporate the washings containing the globules and the remainder of the serum. This is evaporated to dryness and dessicated under the air-pump receiver in the same man-

ner as was the fibrine. Subtract the dry remainder from the weight of the whole clot previously noted, and the remainder will be the weight of the water of the clot. Add this last to the weight of the water of the partial analysis of the serum, and the sum will be the *weight of the whole water* in the whole quantity of the blood analyzed. Then say—

As the water of the decanted serum or partial analysis, is to the dried serum from the same—so is the whole amount of water as above, to the *whole amount of serum* in the quantity of blood analyzed.*

Thus we shall have obtained—

1st. The amount of Fibrine;

2d. The amount of water;

3. The amount of dried serum;

And it is now only necessary to add the weight of these items together, and subtract that amount from the weight of the whole blood analyzed, and the remainder will be the amount of the fourth and last quantity required.

The Globules.—It is well to collect the blood in a thin, light vessel which has been weighed, for this enables one to weigh the whole, and by subtracting the weight of the vessel, obtain that of the blood.

If the air-pump could be dispensed with by ascertaining the proportion of the retained water, at the temperature of the water-bath, and making a suitable allowance for it—a thing which I think practicable—this analysis would be in the power of a much greater number of persons.

ANDRAL'S HEALTHY BLOOD.

Fibrine,	3.00
Blood globules,	117.00
Solids of serum,	80.00
Water,	800.00
Amount	1000.00

* Or instead of applying this proportional formula, the whole quantity of water, when obtained, may be multiplied by the amount of the dry serum in the partial analysis; or multiply by the upper number of the above fraction and divide by the lower number of the same, then will the resulting number be the weight of the whole serum.

ANDRAL'S BLOOD IN DISEASE.*

BLOOD.	Health.	Variation in disease.	Rheumatism.	Fever.	Ammonia.	Cerebral congestion.	Bright's Disease.
Fibrine.....	3	10½ to 1	10	.9	3.5	2.7	3.2
Globules.....	117	185 " 21	101	93.1	38.5	152.3	82.0
Solid Serum.....	80	114 " 57	90	86.0	89.0	125	74.8
Water.....	800	915 " 725	799	820.0	859.0	740	850.0
	1000		1000	1000	1000	1000	1000

* See Bence Jones' Animal Chemistry.

PART SECOND.

AMERICAN INTELLIGENCE.

ART. I.—PRIZE ESSAY— *On the zymotic theory of essential Fevers and other disordered conditions of the Blood.* By SAMUEL G. ARMOR, M. D., of Cleveland, Ohio.

"I profess a liberal medicine: I am neither of the old sect nor new, but follow wherever they cultivate truth."—KLENIUS.

There are few inquiries in pathological science of more interest than those which relate to changed conditions of the blood; for whether we regard it as endowed with a distinct vitality, and obedient to the laws of cellular growth, development and decay; or as ministering to the nutritive and textural wants of the system in the elaboration of fibrine from elements furnished by primary assimilation, or as connected with important chemical changes essential to a healthy action of the system; whether we regard the blood as contributing to one or more of these purposes in the animal economy, it becomes at once evident that destruction of its vitality, or change in any of its constituent elements, must be followed by serious con-

stitutional disturbances. Hence the interest with which its diseases should be studied, and the importance of understanding, in a curative point of view, the primary or secondary impression of disease upon this fluid.

It must be confessed, however, that the question of *priority* or *sequence*, although of much interest to him who thinks or reasons about the origin, nature, and phenomena of disease, is often one of difficult solution. But to arrive at greater certainty on this point, if possible, so far as relates to the Essential Fevers, is the object of this essay; and if I shall succeed, in any degree, in pointing out the distinction between *symptoms* of diseased action and *diseased action itself*, I will have, to some extent, at least, accomplished my object.

In M. Andral's classification of lesions, in which he makes *all* disease to exist, he embraces some in which no notable change of either *organization* or *composition* can be detected. Yet it is worthy of inquiry as to whether this eminent pathologist has not included in his lesions some which are but *symptoms*, not properly diseases—*actions* and not *states*.

It is not my purpose, however, at present, to enter this field of inquiry. I desire to call attention to another question in which no such controversy can arise.

In the essential or idiopathic forms of fever, it is evident that change has been induced in the blood by the admixture of *foreign matters*. The proof of this consists in the fact: 1st, That diseases analagous to those fevers have been induced by injecting putrid matter into the veins of animals; 2d, These fevers are readily produced by the introduction of animal poisons into the blood, as in the case of small-pox, measles, &c.; 3d, These poisons are known to operate through the medium of the air, by thus gaining access to the blood through the lungs; 4th, The non-contagious fevers, such as intermittents and remittents, are universally admitted to depend upon a poisoned or changed condition of the atmosphere; 5th, Actual observation establishes the fact that the blood *is* altered in all essential or idiopathic fevers.

The best point of departure, therefore, is the general fact,—for it should be regarded as such—that all essential fevers depend principally on a poisoning of the blood, and the proof as to primary impression, will be given in illustration of the facts already cited.

It must not be inferred, however, that I am laboring to establish the *identity* of fevers. No such inference can be legitimately drawn from any fact or reason which I shall present. True, so far as the general fact is concerned, that all foreign matters, when introduced into the blood, change either its physical, chemical, or vital properties, all essential fevers may be regarded as a *unit*; yet observation has abundantly established the fact that different poisons act differently on the human constitution, and upon the peculiar and specific character of each depends not only the destructive effect on the blood, but the local lesions that will ensue. Urea and its compounds, if retained in the blood, affect the brain and nervous system, and are apt to give rise to a low grade of inflammation in serous and serofibrous tissues, while mucous structures will suffer but little. But the small-pox virus spends its force upon mucous and cutaneous structures, and leaves, unharmed, the serous and fibrous structures.

There can be no explanation given of this other than the general fact that the tissue or viscus affected seems to be that which has an affinity for the poison which has to be eliminated from the blood. In this process of elimination, inflammation and its sequels are excited, and local disease becomes manifest. Hence all essential fevers should be regarded as distinct in species *according to the circumstance of the primary sedative impression.* This is the only true and rational classification of fevers.

We feel authorized in asserting, then, as a starting point in our reasonings,—what observation abundantly establishes,—that each specific miasm has its own peculiar and distinct law of development. But in the absence of reliable information as to the *essential nature* of these miasms, it would be idle to speculate. Our knowledge on this point must, at least for the present, rest on observation.

But it will be at once perceived that our knowledge of the action of remedial agents is not more certain. Indeed, the perfect analogue of one is found in the other, and the reasoning applied to one applies with equal force to the other. Thus, that mercury will excite inflammation of the salivary glands; arsenic the mucous structures; belladonna the skin; ergot the uterus, &c.—has long been a matter of observation. But why they should do so, is just as obscure as why the typhoid poison should select for its destructive action the

glands of Peyer, or the small-pox should spend its influence upon the dermoid structures. The articles of materia medica furnish a just illustration of the action of foreign substances in the production of disease. Mercury, arsenic, or Croton oil, if uncontrolled by the judicious skill of the physician, is capable of giving rise to diseased action with as much certainty, and as varied in its manifestations, as either of the animal poisons to which I have alluded; and analogy would lead us to suppose that if we could *control* one as we can the other, miasmatic poisons might be used as therapeutic agents. That all agents that affect the vitality or composition of the blood, bear certain general pathological relations cannot be doubted—but that by no means proves the doctrine of identity. As well might we assert the identity of small-pox and typhus fever, from the fact that the fibrin of the blood is deficient in both.

An important point, however, to be established before conclusions are drawn, is the fact that the blood does undergo change in disease, and from medication, diet, &c.; for if this be denied, our conclusions will be without a predicate, and therefore unsound. But the chemists have, happily settled this point, by furnishing us accurate analysis of the blood, both in health and disease. In the condition of health the venous blood of a man, as represented by the number 1000, is composed of

Serum	869.1547
Globules—fibrin included ..	120.8453
	1000

This varies, however, according to age, sex, temperament, kinds of food, evacuations, &c. The rapidity with which some of the solid constituents of the blood are diminished by blood-letting, for example, is very remarkable. Thus, according to the researches of of Dumas, the blood of a robust young man of 23 years of age, gave:

At the first venesection—

Water	780.210
Globules	139.139
Albumen	80.661
Salts	
Fatty and extractive matters	
	1000

At the third venesection:

Water	853,46
Globules	76.19
Albumen, Salts, Fatty and exrtactive matters	70.35
	1000

The more solid constituents of the blood, it will be seen, are rapidly supplied by a compensating quantity of non-sanguinous fluid; and hence the value of blood-letting when it is desirable to promote absorption.

Diet and drinks also very readily affect the constitution of the blood. According to M. Dennis, in the blood of a young man 21 years of age, were found:

Water	770
Globules	154
Albumen, &c	76
	1000

And after 40 days' use of watery drinks:

Water	804
Globules	111.9
Albumen	84.1
	1000

It will thus be seen that the blood is very readily changed in its constitution by blood-letting, diet and exercise. It sustains direct relations also, to the air we breathe, to the water we drink, to the food we eat, and to the excretions of the body by which it is purified; and that a fluid which is presented to us in such a compound and complicated form, and sustaining so many relations to the various modifying influences which surround it, should not become a frequent seat of *disease*, would indeed be an anomaly in nature.

Among the various hypotheses to account for fever, a zymosis, or fermentation of the blood, has prevailed under one form or other from remote antiquity. But it has been so inseparably connected with the old humoral pathology, that it has received but little consideration. Recently this hypothesis, (for I shall regard it as such at present,) has been rendered, to say the least of it, very plausible by the researches of the distinguished Liebig.

In his "Animal Chemistry" he calls attention to the fact that no other component part of the organism can be compared to the blood in respect of the feeble resistance it offers to exterior influences, and the reason assigned is, that "it is not an organ which is formed, but an organ in a state of formation." The following quotation embodies in a few words, the main leading thought of the author on this subject:

"The chemical force and the vital principle hold each other in such perfect equilibrium, that every disturbance, however trifling, or from whatever cause it may proceed, affects a change of the blood."

This, then, is an important starting point in our reasoning process; for if it be really possessed of a low vitality, we may logically arrive at the conclusion, by *a priori* argument, if we knew nothing of the facts in confirmation of it, that all fevers, produced by endemic, epidemic, or infectious causes, have their origin in a primary diseased condition of the blood.

A zymotic change of the blood is due, according to Liebig, to a decomposing organic molecule in the interior of the human body. This molecule, by a law of analysis, induction or contact, has the power of imparting its own motion to another molecule with which it may be in contact; hence chemists have defined it to be "decomposition by contact," or the "action of presence." We have illustrations of this law in the power which small quantities of substances, in a state of change, possess of causing unlimited quantities to pass into the same state; and it is an interesting fact, worthy of note in this connection, that all substances which readily suffer this transformation are, without exception, bodies which contain *nitrogen*. A large portion of the blood being composed of this element, we might readily conclude that it was the vital principle alone that keeps it from spontaneously passing into this condition of transformation. If the catalytic force be greater than the resistance offered by the vital principle, the blood must pass into a condition of decomposition.

It may be asked however, with reference to this law of zymosis or induction—Is there any evidence to show that the introduction of putrid matter in the animal system, does give rise to effects which are at all comparable to those of fever? If not, the law which has been

announced is but speculation, and at best an hypothesis. But let us see. "It is a fact," says Liebig, "that subjects in anatomical theatres frequently pass into a state of decomposition which is communicated to the blood of the living body." And the fact observed by Magendie, that putrifying blood, brain, eggs, &c., laid on recent wounds, cause vomiting, lassitude and death, after a longer or shorter interval, has never as yet been contradicted. Numerous experiments have demonstrated that putrid matter, injected into the blood of healthy animals, will give rise to a set of symptoms which are very analogous to Typhus. "If a small portion of putrid matter," says Armstrong, "be accidentally introduced into the blood during dissection, or if the experiment be made upon lower animals, it produces fever, having exactly the character of Typhus, under its continued form, and *no individual could confidently pronounce that it differed from it.*" Bernard has also shown that by injecting yeast or sugar into the circulation, many of the ordinary kinds of fermentation are excited, giving rise to a disease very analagous to Typhoid fever, accompanied by prostration of the strength, bloody fluxes, ecchymosis, and a black and uncoagulated condition of the blood. "Lastly, it is," says Leibig, quoting from Henle, "a universal observation, that the origin of epidemic diseases is often to be traced to the putrefaction of large quantities of animal and vegetable matters; that miasmatic diseases are endemic in places where the decomposition of organic matter is constantly taking place, as in marshy and moist localities; that they are developed epidemically, under the same circumstances, after innundations; also in places where many people are crowded together with insufficient ventilation, as in ships, prisons, and besieged places."

It is also worthy of note that these factitious fevers, produced by the introduction of deleterious substances directly into the blood, are analogous, both in their symptoms and pathological lesions, to those produced by the sting or bite of certain animals; they present also the same general class of symptoms that are present in small-pox, malignant scarlatina, and other eruptive diseases.

In Mr. Walker's work on Grave-yards, he also presents an array of facts which prove, beyond all controversy, that putrid animal exhalations have given rise to diseases that have raged like a pestilence

or epidemic. He cites an instructive instance which occurred in 1733 in the parish of St. Saturnine, in Burgundy. A sexton, while letting down a corpse into the vault, accidentally broke a coffin which contained the body of a fat man that had been buried twenty-three days. A discharge of sanies followed, which greatly annoyed the assistants; and "of one hundred and twenty young persons who assembled to receive their first communion, all but six fell dangerously ill, together with the Curè, the grave-digger, and sixty other persons." The disease is described as a putrid, verminous fever, accompanied with hemorrhage, eruption and inflammation.

Facts in support of these views might be accumulated at great length. Dr. Francis Home communicated measles by means of a drop of blood from a patient affected with the disease. And the experiments of M. Gendrin, as given in Williams' Principles of Medicine, is a striking one in point:—A man who had been skinning a diseased animal, was seized with a putrid fever, attended with an eruption of sloughing pustules. Some blood taken from this man was injected with the cellular texture of the groin of a cat; the animal was soon after affected with vomiting of bile, dyspnœa, frequent, small, and irregular pulse, dry, brown tongue, slight convulsions, and died seven hours after the injection." The same pathologist induced in animals various and severe symptoms, followed by death, by injecting into their veins the blood of persons laboring under Small Pox. M. Dupuy and Lauret also communicated the malignant pustular disease known as "Charbon," by injecting into the veins of the healthy horse a minute quantity of blood of the diseased animal. Andral relates an extraordinary case in which a malignant fever, followed with pustular eruption and death, was occasioned by the mere contact of the lips with the diseased blood of an animal.

May we not, then, infer from these facts, that the blood is the hot bed in which many malignant diseases are propagated, whether by ova, parasites, cell germs, or zymotic action?

But our proof does not rest here. Clinical observation has long since established the alteration of the blood in diseases which are termed *putrid:* the blood appears to be in a partial state of *dissolution:* its vitality is destroyed, and its fibrin either not elaborated, or dis-

solved in the process of putrefaction. As a result of this decomposition, an increased quantity of hydro-sulphate of ammonia has been found in the blood of patients suffering from Typhus and other malignant diseases; and hence the *alkaline* reaction of the urine that is so often observed to be present in these fevers.

These observations have been made the basis for the support of a great group of maladies which go by the name of Zymotic diseases, and include, according to the statistical nosology of Mr. Farr, small pox, chicken pox, all eruptive diseases, influenza, scurvy, purpura, ague, remittent fever, yellow fever, typhus, puerperal, plague, hospital gangrene, &c. And in proof of their Zymotic origin, the fact has been offered—1st, That the vitality of the blood is low, and that it therefore readily suffers transformations; 2nd, That we can produce in animals and man, factitious diseases by inoculations or injections of putrid or contagious matter, having all the characteristics of the Essential fevers; and 3d, That clinical observation establishes the fact that the blood is changed. And that the febrile phenomena present in these fevers indicate a condition of the system independent of inflammatory action, I infer from the fact—1st, That in the absence of complication during the progress of the disease, there is no evidence of inflammation revealed by post mortem inspection; and 2nd, That the symptoms coëxist with a diminution of the fibrin of the blood, and diminished tolerance of the loss of blood. In some of the most malignant forms of fever—those in which the fibrin of the blood is at its minimum—there is often not much heat of the body and but little increase of pulse. The patient often dies in the cold stage of such fevers, without in fact having any fever! Evidently, therefore, the term *fever* may be used in two very different senses—in one signifying a collection of symptoms depending on local inflammation—and in the other a *condition* of the system entirely independent of such inflammation. In one the term indicates the name of a *disease*, and in the other the name of a *symptom*. Hence the distinction between essential and symptomatic fevers, and hence the inference, also, that essential fevers have their origin in certain qualitative changes of the blood, caused by the introduction of foreign matters.

Further proof that the general class of diseases which have been termed zymotic have their origin in the blood, is drawn from the

symptoms usually present. These will be found accurately detailed by all the standard writers on General Pathology under the head of "*Necræmea, or death beginning with the blood,*" such as petechiæ and vibices on the external surface, the occurrence of hemorrhage in internal parts, the general fluidity of the blood, its frequently dark and otherwise altered aspect, its proneness to pass into decomposition, the general prostration of all the vital powers, the dark tongue, sordes on the teeth, suspended secretion, and the general arrest of molecular nutrition. Indeed, the very *universality* of diseased action points to a cause more general than can be found in any individual function.

I have thus far spoken of the admixture of foreign elements in the blood from *without*. There are causes, however, which operate on it *intrinsically* as well as *extrinsically*. Thus defective excretion is followed by a direct *backward* action on the blood, resulting in changes of its chemical or vital properties. The excretory organs are the natural emunctories through which effete matters generated within the organism, are expelled from the blood; the product of the various excretions may be regarded, therefore, as the correct expression of the numerous changes that are taking place both in the healthy and diseased animal fabric. In febrile diseases these organs are generally suspended in the exercise of their healthy functions—an increase of perspiration, or in the flow of urine, or a spontaneous diarrhœa, being generally accompanied with a subsidence of the febrile phenomena.

Relatively considered, the *Kidneys* may be regarded as the most important emunctories through which morbid matter is expelled from the blood. The experiments of Orfila on this subject are highly satisfactory. He found that the pernicious effects of small and repeated doses of arsenic could be readily averted in animals by giving them, at the same time, a diuretic medicine; and the converse fact has been frequently observed, namely, that persons who suffer from disease of the kidney, by which its function is impaired, very readily contract infectious diseases, and are apt to suffer from their effects. It has also been observed that opium, arsenic, mercury, &c., operate with dangerous energy on such patients.

The experiments of Dr. Golding Bird are very conclusive on this subject. His observations have been extensive and accurate. Two,

of many cases, are here given. In the first, a case of ague, the patient was kept in the hospital from the 23d of May to the 16th of June following.

The following is his table of analysis:

May	23.	Passed	12	oz.	urine, and	352	grs.	solid constituent.
"	26.	"	40	"	"	828	"	
"	28,	"	35	"	"	725	"	
"	30.	"	48	"	"	1054	"	
"	31.	"	45	"	"	743	"	
June	2.	Passed	35	oz.	urine and	514	grs.	solid constituent.
"	4.	"	30	"		879	"	
"	6.	"	27	"		1036	"	
"	7.	"	35	"		436	"	
"	9.	"	40	"		1172	"	
"	11.	"	45	"		742	"	
"	13.	"	40	"		916	"	
"	14.	"	43	"		984	"	
"	37.	"	37	"		1045	"	

There was a decided improvement, says Dr. Bird, cn the 30th, severe paroxysms on the 3d, better again on the 6th, and no return of the ague after the 9th.

In the second case of ague—girl aged 19—patient was kept in Hospital from 23d of June to May 7th. On the 23d of May there was a severe paroxysm, and the amount of solids excreted was 280 grs. On the 7th she had a return of paroxysm, and there were 280 grs. solid constituents in the urine. On the 28th she was better, and the solid constituents in the urine amounted 538 grs. On the 30th they amounted to 625 grs., and from this time the patient rapidly got well.

In this, also, it will be observed that *pari passu* with the patient's improvement there was an increase of the solid constituents of the urine. In Typhus, and other adynamic forms of fever, the same facts have been observed,

Are we to infer, however, from these observations, that defective excretion is the primary *cause* of the fever? By no means. That

would be an imperfect view of the pathology of fevers. I can conceive of no instance in which a lesion of secretion can be properly classed among the *primary* elements of disease. Some change must precede it, either of structure, of inervation, or of the blood from which the secretion is formed. The examples cited simply show the curative effects of the removal of foreign matters from the blood by depurating organs, and at the same time go far towards establishing the ancient doctrine of *critical discharges.*

They also serve to point out the two causes of disease which constantly present themselves for our consideration—causes *extrinsic* and *intrinsic.* Of these one or both may be in active operation. It is an erroneous dogma to suppose that but one poison can act on the system at a time. As rational would it be to suppose that but one medicinal agent can produce its effects upon the constitution at a time.

I have already alluded to the fact, demonstrated by observation, that different poisons spend their influence upon different tissues of the body. It is an unwarrantable speculation to transfer this law of *specific contamination* to the structure of the blood, and thus explain the action of different foreign bodies upon that fluid? Some poisons may be regarded as comparatively innoxious; they emerge from the body unaltered with one or more of the ordinary secretions; others destroy some element of the blood, (or its corpuscular element in the lymphatic glandular system,) so that it is never again subject to the same poison, as in the case of some of the eruptive and contagious diseases; while others, by entering into chemical union with one or more elements of the blood essential to life, destroy its vitality, and general necræmia and molecular death soon follow.

This will be recognized as an unproven speculation of the old fashioned humoral pathology. And, in the absence of positive proof, I do not, of course, offer it as one of the "fixed facts" of medical science. Yet observation, reason, and analogy, throw around it a *plausibility* that entitles it to a still further investigation.

In submitting these views I would not be understood as attempting to sustain an exclusive humoral pathology; nor would I underrate the importance of pathological changes in the solids. With Bichat, I regard "every exclusive theory, whether of humoralism or

solidism, as a pathological absurdity." The relation between them is too direct and intimate ever to be separated; neither can the one or the other ever be regarded as an "exploded system." Humoralism will never be exploded as long as the blood is the source of life to the tissues; nor will solidism be disregarded as long as the tissues continue to undergo changes from altered conditions of the blood. My object is rather to fix the mind upon what I regard as an important truth, viz: that in all essential or idiopathic fevers, changes of the solids depend upon previous alterations, quantitative and qualitative, of the blood.

This altered condition of the blood is soon made manifest by general febrile phenomena. The nervous centres, depending directly for their powers on the state of integrity of this fluid, become perverted and weakened in action; the functions of the animal and organic life are depressed; passive congestion, induced by depression of nervous power, follows; and hence the torpor and arrest of glandular action, and the sluggish and languid state of all the functions so characteristic of these fevers.

The importance of all these conditions I would by no means underrate. They present to us, indeed, an exceedingly interesting field of inquiry. From an impression first made upon the nutritive and assimilative functions, we are at once introduced to multiplied elements of disease. There is not a function, not a nerve, not a gland, nor a capillary vessel of the body but must feel the depressing effects of a contaminated condition of the blood. And this sluggish and languid state of the excretory organs becomes the cause of a still more poisoned condition of the blood, until this source of life becomes itself dead, and spreads death instead of life through the body.

The admittance of the zymotic theory into the field of pathology, would doubtless lead to greatly increased knowledge of the real nature of diseased states. In a large class of fevers it points out the only two modes of cure: 1. To counteract the injurious operation of the poisons; 2d. To expel them from the system. The first of these indications is carried out in low typhus and adynamic forms of fever by the administration of saline medicines, such as the chloride of sodium, the chlorate of potash, hydrochloric acid, &c. Arsenic,

quinine, and other antiseptic remedies are also valuable agents in arresting the zymotic condition of the blood.

The other indication is the one most usually pursued, viz: to expel the offending matter from the system. This may be said to be Nature's mode of cure; and in the absence of reliable knowledge as to the nature of the poison and its antidote, the physician can only aid Nature in her work of elimination. This he attempts by the administration of tonics, stimulants and depurents. The powers of life must be supported while nature effects the cure. But even in her own work of depuration, Nature may be greatly aided. The kidneys, skin, and alimentary canal, are the principal channels through which foreign matters are expelled from the blood; and hence the utility of diuretics, aperients, and the so-called *Water-Cure*. The latter, by a combination of diuresis, and diaphoresis, may be rendered a most powerful therapeutic agent in cutting short a fever in its premonitory stages or at its final accession. The absurdity of hydropathy, as a one-ideal system of cure, is its blind and indiscriminate application to every variety of disease; and it is to be regretted that a remedy of such valuable therapeutic power is often brought into undeserved disrepute by falling into the hands of ignorant intermeddlers with nature.

If the views of the pathology of essential fevers which I have presented be correct, it is almost impossible to avoid giving assent to the doctrine that regards the fever as an effort of the "vis medicatrix," instituted for the purpose of expelling the poison from the system, and while it repudiates the doctrine of the boasting fever-curer, as well as the doctrine of non-interference, which has aptly been styled a "meditation on death," it rests upon the great physiological truth that Nature is ever active in her recuperative powers, and is, after all, the best and wisest of physicians.

This pathology also offers the most rational explanation of the modification of the diseased action growing out of prevailing epidemic influences. It fixes the mind upon two controling elements of disease; 1st, Upon the depressing effects of a zymotic poison, by inducing changes in the blood; 2d, The local inflammatory or functional complication that may be engrafted upon and influenced by this altered condition of the blood. It thus draws the line of de-

marcation between general and special pathology, by keeping in view *constitutional conditions as modifying local action*, and by this means enables us to comprehend the most important question, practically considered, within the wide range of medical inquiry—the distinction between *Sthenic and Asthenic* diseases, between depressed and exalted action. Its tendency is to give us broader views and clearer conceptions of the varied elements of disease as they act and react, and insensibly shade into each other.

It modifies, moreover, and renders more rational, the *treatment* of disease. Does an inflammatory affection overtake one whose blood is contaminated by an epidemic influence, or by putrid emanations, vegetable or animal; or whose blood is only imperfectly or badly repaired by insufficient or unwholesome diet?—then of course the inflammation is *asthensic* in character, and other antiplogistic remedies, must be resorted to, if at all, with great caution. If the blood has been "touched corruptibly" by an epidemic influence or typhoid poison, we must husband rather than depress the flagging powers of life. To adopt, in such cases, (as we fear is too often done,) the ordinary treatment for an acute pnemonia or dysentery, as the case may be, regardless of the evidence of *blood disease*, such as loss of tone and strength of the vascular system, sluggish functions, dull mental faculties, feeble and compressible pulse, and brown or dark tongue, would be to hasten the dissolution of the patient, and bring both doctor and medicine into disrepute. The great secret of success in the treatment of disease is to be found in a broad, comprehensive and rational pathology,—a pathology that weighs every element of disease, whether of fluids or of solids, that is capable of exciting, depressing, or perverting vital actions.

I have thus ventured to present a few facts with the hope of calling attention to a subject which may not as yet, have occupied the minds of some of the members of the profession; and although I may have presented but a dim vision of the true light, yet *I trust I have got hold of my pitcher by the right handle.*

ART. II.—*Extracts from the Records of the Boston Society for Medical Improvement.* By WM. W. MORLAND, M. D., Sec'y.

SEPTEMBER 27, and OCTOBER 11.—Cases of Arrest of Development, Blemishes, etc., in Infants, attributed to Strong Mental Action in the Mother, while

Pregnant; Disagreeable Sights; Sudden Shock, &c.; Discussion upon the Question whether such Action of the Mind be ever a Cause of such observed Results.

Case I. *Arrest of Development attributed by the Mother to a Fright during Pregnancy.*—Dr. Storer reported the case. Several months since, a female patient of Dr. S.'s expressed some anxiety respecting her sister, who expected shortly to be confined, and who could not be persuaded that she would not have a deformed child. Three or four weeks after her marriage, she was much affected at seeing a hen injured by a stone thrown by a boy—the stone broke one of its legs and removed the lower portion. She was exceedingly troubled at the time, and as her pregnancy advanced, continually dwelt upon the subject, insisting that her child when born would be found to be deformed. Her friends at last began to feel some concern as to the result, and hence the reason of Dr. S. being consulted. A few weeks afterwards, Dr. S. heard from his patient that her sister had been confined, and that one foot was wanting; that, immediately after her delivery, the mother asked to see her child; one foot was shown her as being natural, and an effort made to divert her attention from the child; but she could not be pacified until she saw the other limb, continually insisting that she knew it was deformed. Dr. S., being very anxious to see the child, requested that he might know when it should be brought to this city.

Since the last meeting of the Society, he has had an opportunity to examine it. The child, perfectly well formed in other respects, exhibited upon one of the lower extremities, simply a heel and the rudiments of the five toes, at the extremities of which were placed microsopic nails. Dr. S. observed that he was led to refer to this case from the peculiarities of the patient's conduct previous to her confinement. It was not an uncommon circumstance for a mother, *after* the birth of a deformed child, to account at once for the defect by something which had occurred during her pregnancy; but he believed it to be very rare for a patient to dwell upon the subject for months *previous* to her confinement, and immediately upon her delivery insist upon seeing her offspring to prove her conviction.

Dr. Bigelow, Sen., remarked that very many of these cases are referable to afterthought of the mother. Women are often anxious in

regard to the possible deformities of their offspring, and there are few pregnant women who escape seeing some peculiar and even disagreeable sight or objects during their gestation; any marks observed would, therefore, be coincidental *by the rule of chances:* in ninety-nine out of a hundred pregnant females, no untoward results would be observed. Dr. B. does not believe it possible that an arrest of development can arise from the action of the imagination of the mother, or from the effect of sudden disagreeable impression on her mind. It is not unfrequent that mothers inquire *whether their child be perfect;* at all events, the *frequency* of deformities after any shocking sight is the *test* of the truth of the existence of such *cause* for deformity. Dr. B. believes that if one hundred pregnant women were to be exposed to the action of such supposed causes, or should experience any of the strong and peculiar "longings" of pregnancy, no blemish would be thereby produced in the children.

Dr. Cabot referred to the fact that, during the siege of Antwerp, there were very many *stillborn,* and a large proportion of *deformed* children.

Dr. Durkee asked, if this theory of the influence of mental impressions on the pregnant female does not find support in certain analogies observed among animals? He referred to the Old Testament narration (*Genesis,* chap. xxx,) and added that remarkably similar facts may be observed at the present day in the breeding of animals.

Dr. Jackson said that very likely there might not be one in a thousand, exposed as suggested by Dr. Bigelow, in whom unfortunate results would follow; *all* are not *equally susceptible* of such influences—some not *at all so;* the *special* cases, however, are not to be explained or ridiculed away; there is enough of plausibility, if nothing more, in the view he sustains of the matter, to render women cautious of needless exposure to unpleasant sights in the *early months* of pregnancy.

Dr. J. added that, from the cases he had seen, or heard of upon good authority, he was inclined to believe that a malformation in the fœtus may be induced by an external cause operating upon the mind of the mother during her pregnancy, and, further, that the malformation may bear to the cause some relation or resemblance. This

last has always been a popular notion; but is regarded, on the other hand, by the scientific, as perfectly absurd; the presumed cause operating upon the mother at a period of pregnancy when the fœtus must be too far developed, it is said, for any such malformations as have been referred to, to be produced. There is, moreover, no nervous communication between the mother and the fœtus. The number of negative facts is numerous beyond account; of cases, that is, where a sufficient cause, according to the above hypothesis, existed for malformation, and yet none occurred. No possible explanation can be offered for the production of a malformation by the presumed cause; nor will it be possible, until we know much more than we do of the obscure subject of the physiology of the fœtus. The resemblance between the malformation and the cause, it is said, is a mere coincidence; but, upon the common doctrine of chances, the coincidence is too remarkable to be explained away so readily, and, if one case is suggestive, a second adds very great weight, and a third is almost or quite conclusive. M. St. Hilaire (*Anomalies de l' Organisation*) believes that some monstrosities, the anencephalous for instance, owe their being, in some cases, to the operation of a strong moral cause upon the mother; but he does not believe in any resemblance in the particular malformation to the exciting cause, and for the reason above mentioned. It would have been well had he stated at what period of pregnancy the mothers, in his cases, were subjected to the causes that led to their monstrous births. Another remark often made, is, that pregnant women do not predict the malformation, but, when it is discovered, look back for some cause to which they can refer to it. This might be questioned; but, allowing that it is so, and even that they had ceased to think of the assigned cause, it does not follow that it had not operated efficiently at the time; the impression was made upon the mind of the mother, and through it upon the fœtus—the one was soon effaced, the other remained. Much would depend upon the character of the exciting cause, and much also, as he had before remarked, upon the *susceptibility* of the individual to the receiving and the retaining of impressions. Dr. J. then reported the following cases:—

1. Several years ago, he saw a young lady, about thirteen years of age, of rather a small and delicate figure, and whose middle finger

upon one hand was fully equal in size to that of a man's; the corresponding metacarpal bone being also decidedly enlarged, and even the forearm somewhat so. The finger was itself well formed, as in some other cases of similar malformation that have been observed here. The hand also was otherwise well formed.

The mother of this young lady, during her pregnancy, was obliged for a long time to dress a felon or whitlow for an old uncle, the finger affected corresponding to the one that is malformed in the case of her daughter; and the operation was always disagreeable to her, as she was a woman of a particular nervous temperament.

2. In November, 1846, Dr. J. saw an infant with an extensive blood-mark, as it may be called, upon one of the upper extremities. The mother, whom he has attended for several years, and knows to be a woman of very susceptible feelings, gave the following statement: When two and a half to three months pregnant, though not at the time aware of her situation, she had been out of town for the afternoon, when, on her return, as the cars entered the depot, she saw a man who had been recently killed. He was lying upon his face, with his head turned to one side, the sleeve of his coat torn entirely off, the whole upper extremity exposed and more or less bloody, and so twisted, as she supposed from dislocation at the shoulder, that the hand lay upon the upper part of the back. At this sight she nearly fainted, and probably would have entirely, if she had not had two of her children with her. For at least a month afterwards she was decidedly ill; and during the remainder of her pregnancy she could not banish the thought from her mind; she also felt that there would be great danger in regard to the child in utero, though she never expressed her anxiety upon this point to any one. The extent and degree of the discoloration of the skin were not fully recorded by Dr. J. at the time; but, having recently seen the mother, she informs him (for the child has since died) that it extended from the back of the hand and towards the thumb, where it was most marked, upwards along the extremity to the shoulder, and even somewhat upon the neck. It was not equally marked throughout, being in some parts more or less continuous, and in others simply spotted or dotted. Her other children she has always dressed in short sleeves; but this child's arms she covered, on ac-

count of the marked deformity; and, as she says, if it had lived, it could never have worn a low-necked dress. The child itself, moreover, was so sensible of the mark that it would occasionally be seen endeavoring to wipe it off. The discolored arm in the child corresponded, as to the side affected, with the one that was injured in the man.

3. Dr. Z. B. Adams related to Dr. J., several years ago, the following case: He attended a woman in labor, and the child was found to have one of the forearms terminating in a conical stump just above the wrist. During her pregnancy she attended for several weeks upon her brother who had had his hand torn off by machinery, and the forearm afterwards amputated; the injury being upon the same side as the malformation in the child.

4. Case reported by the late Dr. Doane, of this city. The mother of several blind children was holding one of them in her lap, when it put its finger into one of her ear-rings, drew it down forcibly, and tore it through the flesh. She was pregnant at the time; and when her child was born, it had a fissure in the ear corresponding to the laceration in its mother's.

At the next meeting, Dr. Storer said he had met with reports of two cases since the last meeting, which tended to confirm the opinion at that time advanced, that any remarkable sensation sustained by the mother during her early pregnancy, might produce an effect more or less well marked upon the fœtus in utero—that this effect need not necessarily partake of the character of the exciting cause—but that, should the mother be so far influenced as to receive a decided shock upon her system, it might be followed by some abnormal condition of her child—arrest in development or some peculiar malformation. The following cases he had observed in the "Proceedings of the Medical Association of the State of Alabama," for December, 1850.

"Dr. R. Lee Fearn related the following very remarkable particulars of a case, where the impressions received by a mother during pregnancy, affected her child in utero. A gentleman, whilst shooting, shot through the metacarpal bone of his index finger. The wound was a bad one, and piece after piece of the bone came away. A few months after the accident here mentioned, and in due season,

his wife bore him a child perfectly formed in all respects. When about four months advanced in her second pregnancy, an operation was deemed necessary to remove the last remaining portion of bone in her husband's finger. She witnessed the operation, and was much shocked and alarmed at the sight. When her child was born, it was found to be deficient in this very bone, though in all other particulars it was a well-formed child. The Doctor thought this was by no means the result of chance, but a very conclusive instance of cause and effect.

"Dr. Dossey remarked, that the relation of this case called to his mind a similar instance:—

"Dr. G—— was thrown from his horse and broke his leg, midway between the ankle and knee. His wife was about five months advanced in pregnancy. When the child of which she was pregnant was born, it had on the leg corresponding with the injured limb of the father, and at precisely the same spot, the appearance of a fracture of the limb, and there was also a decided shattering of the leg."

Dr. Strong thought that the notion so popularly entertained in reference to this matter is too general not to have *some foundation in truth.* It certainly deserves investigation. He added, that the idea of greater liability to these attributed effects, from the causes mentioned, in the *early* part of pregnancy, seemed to him very well founded; perhaps the influencing power, be it the imagination, or what it may, *ceases after a certain period of gestation;* has a *limitation.* Dr. S. also referred to the fact that a *mare* covered by a *quagga* always afterwards, when impregnated by a *stallion,* brought forth *striped colts.*

Dr. Cabot mentioned the statement of M. Donne, of Paris, who declares the above fact, as stated by Dr. Strong, to be constantly observed under the same conditions; he even goes farther, and asserts that one portion of the constitution of one man is propagated, *by another man,* from *the widow of the first to children by the second.*

Dr. Bigelow, Sen., asked to what cause we must attribute the imperfections and arrests of development observed in fruits, flowers and plants. Have *they* imagination? and does it act with this effect upon them?

Dr. B. declared his entire disbelief in any such effects from the attributed causes.

Dr. Hayward, Sen., asked whether this action or influence (if it be recognized as efficient) is *imaginative* or purely physical? Dr. H. mentioned a mother, who, while pregnant, suddenly saw one of her children, ill with pneumonia, covered with blood by some accident; the child she was carrying, when born, had a *large red stain* or spot upon its face. Here, imagination could hardly be the cause, for the woman had not thought of, or brooded over, the occurrence, nor had she any apprehension that her infant would be marked.

Dr. Homans spoke of an acephalous fœtus, the mother of which during pregnancy had not even apprehended any marring of her child. Dr. H. does not remember, since that case, any other wherein the mothers had *anticipated* any monstrosity although such did occur. These facts he considered would militate with the imagination doctrine, or the idea of effect from strong mental impression or shock on the fœtus in utero.

Dr. Strong remarked upon the difference between mental and physical laws in their action on the system. Some individuals have, and some have not, the imagination sufficiently sensitive and impressible for the production of such marked action as has been referred to. The *final question*, however, is whether such results are *ever* produced. If this be proved and conceded, it is sufficient to make the fact a fixed one. *One* instance thus established is enough, and as good as *ten thousand*. The numerical system is valueless upon this question.

Dr. Bigelow, Sen., would pronounce this principle of different impressibility, &c., to be the foundation of all the charlatanry of the day. The remarks of Drs. Jackson and Strong go to prove only the possibility of these occurrences under the above supposed theory, but do not establish their certainty. There is not sufficient basis, as he views the subject, for making a rule or law in regard to it.

Dr. Strong asked if any one had seen so strongly marked special cases as that of the slitted ear and the enlarged finger narrated by Dr. Jackson?

Dr. Hayward, Sen., mentioned two cases of slitted ear; the slit being in the portion in which ear-rings are inserted. One individual, when seen by Dr. H., was 16 or 17 years of age. The mother did not remember any accident or sight possibly causative of such a result.

Dr. Wm. T. Parker related the following case which occurred in his father's practice in Virginia, many years ago: A lady, three months advanced in pregnancy, saw a pig, driven furiously out of an inclosure by a negro boy, have *its bowels torn out* by the stake of a fence. The lady was greatly shocked, and fainted. Her child, when born, had the entire front of the abdomen *covered only by a thin film, and the intestines were visible through it.* There was also imperforate anus. The child died not long after birth.

Dr. Channing related the case of a lady in Edinburgh, of which he heard while there lately: Her surgeon had been performing the operation for hare-lip, and the lady's attention was drawn to some blood which chanced to remain upon one of his fingers; at her request, he described the operation to her. She was from four to five months gone with child; at birth, *the child was found with hare-lip.* The mother stated that she had been much impressed by the above occurrence and narration.

Dr. C. alluded to the fact that the umbilical cord sometimes performs amputation of fœtal limbs.

Dr. Coale said that the agency of mind upon matter cannot be denied. Sometimes the cause and the effect of nervous impressions are greatly disproportionate the one to the other. The action of these impressions on the stomach and bowels is undoubted. Dr. C. mentioned a patient under his care who, during her pregnancy, saw a deformed man, and was greatly affected and troubled at the occurrence. Previous to her confinement, however, the vivid impression subsided, to be renewed during the throes of parturition. She became very apprehensive of possible deformity of the child about to be born, which, however, presented none.

Dr. Bigelow, Jr., thought the *negative,* as well as the *positive* evidence on this subject should all be weighed; otherwise, *fallacious results* are nearly sure. *Strong numerical evidence* and *many cases* seem to him necessary for a decision; isolated cases are insufficient.

At the meeting holden Nov. 8, Dr. Storer reported that he had, during the last week, delivered a woman of an infant with hare-lip; the mother, the day after confinement, told him that she had been very painfully impressed while visiting, during her own pregnancy, a friend who was dying from phthisis, and who, during her visit, had a severe and sudden attack of hæmoptysis, accompanied by dis-

tressing dyspnœa. The pregnant lady was greatly shocked, so much as to faint; and she subsequently thought frequently of the occurrence, fearing its effects upon the child she carried. This infant was born with a hare-lip of double fissure.

Dr. Gould mentioned, as a "set-off" case to the above, the following: On last Monday, he attended a woman in labour, who, at the birth of her child, was very anxious that its palate should be examined, she having, early in her pregnancy, seen a person with very disagreeable countenance from deformity of the palate. She had been very unpleasantly affected by the sight, but, notwithstanding her very strong apprehension, no effects are visible upon her child.

The discussion of the subject was not resumed.—*Americaa Journ. of Med. Sciences.*

From the Southern Medical and Surgical Journal.

ART. III.—*Another White African.*

WHITEVILLE, Harris Co., Ga., May 16, 1853.

Dr. Dugas: Dear Sir—I noticed in the Eclectic department of the April No. of your Journal the report of a case of "Change of Color in an adult negro." I propose, briefly, to present you with a case of the same kind, equally as remarkable and much nearer home.

Charlotte, is a woman 34 years old, and living within one mile of me, whose skin is as fair as that of any lady of Caucasian blood, and who was as black at the age of 11 as any African. She says her health has been uniformly good, with the exception of one "spell of bowel complaint," which occurred when she was about 10 years old; sometime after which a white spot appeared on her forehead, which gradually though slowly enlarged. In the mean time other spots appeared on different parts of the face, which also increased in size, until the whole face became perfectly white. The change in the color of the face was completed in about six years, and she says that after her face "turned white," her whole body changed in one week. I saw her frequently during the metamorphosis, and noted its progress, during which time her countenance was so hideous that she was a fright to all the juveniles of the neighborhood.

She is at present strictly a white woman, except her eyes and hair, which are those of the negro—the latter, however, being whitish all around the margin. The skin freckles, and is easily blistered by the sun. She is a good servant; has ordinary intelligence; has had ten healthy children, all of whom are as black as their father, who is a full-blooded African.

The above are the facts of this "strange freak of nature." Was the change a pathological or a physiological one? I am inclined to the latter opinion, because the subject was in good health, and the skin seemed to be so, during the change. But what strange whim possessed the absorbents to remove the pigmentum nigrum?

These are questions I shall leave to be answered by those who have more taste and talent for speculation than your humble servant.

E. C. HOOD, M. D.

Art. IV.—*Quinine in Cholera Infantum.* By G. W. Booth, M. D., of Carrollsville, Miss.

As the season is near at hand when that scourge of infancy, Cholera Infantum, usually makes its appearance, I will again invite the attention of the profession to the views I entertain of its etiology and treatment as published in some of the medical journals in 1851. In the communication referred to, I stated it as my opinion that it was of malarial origin. There are many reasons that I could give to sustain the correctness of this opinion, but I deem it unnecessary at this time.

My views respecting the origin of the disease influenced me to use the great and approved antiperiodic, Quinine, in its treatment. The success I met with in combating the disease with this article still farther confirmed me in my convictions as to its predisposing cause. I earnestly solicit the profession to give Quinine a fair trial in Cholera Infantum: I feel confident that no one will regret the experiment. I use other articles to meet particular indications, such as acetate of lead, and calomel, in small doses, for controlling the discharge from the bowels, acting on the liver, &c.

ART. VI.—*Cold Water in Dysentery.* By F. BLADES, M. D.

If it be the accumulated experience of individuals which gives us our *rules* in the practice of medicine, every one ought to contribute his mite, if it be of any value. I am, therefore, prompted to send you a slice of my experience.

Last year I had many cases of dysentery to treat. Some of these "wore the livery" of the ordinary non-malignant variety, and were amenable to the usual remedial means; while others—the majority—were of the epidemic malignant variety, and with surpassing stubbornness "went their ways," heedless of cure, i. e., by the mostly practised methods.

Now, we, who have not a reputation to live on after defeat, cannot well afford—if I may use a sinister expression—to lose many patients consecutively, else we fall into disrepute and straight-away lose our practice.

This motive, which was secondary to the heartfelt interest I had in the recovery of my patients, as also this latter motive, caused me to depart from the calomel and opium, etc., etc., *land-marks*, in treating the more malignant variety of dysentery. I have now in my mind a case which, conjointly with Dr. Fowler, my partner, I was called upon to treat. The malady "waxed exceeding sore" from its outset. The griping was positively excruciating; the straining extremely ardent and incessant; the stools exceedingly large, grayish and bloody, containing membranous-like shreds; the pulse was quite frequent and forcible. This is a rudely sketched outline of the condition of the case as reported to me to have existed prior to my attendance. The doctor who was first called had treated the case with calomel and opium, q. s., castor oil and laudanum, as a laxative, once in twenty-four hours, with other adjuvantiæ now passed from memory, for three or four days, at which time I was called to see this case with him. The above mentioned symptoms were said to be unabated. The pulse was now feeble and about 120; the tongue was covered with a thick brown fur, and dry, the edges were fiery, and the whole tongue was dotted over with elevated papillæ—here and there protruding through the fur coat. The stomach was so excessively irritable that it would scarcely retain a tea-spoonful of water. I suggested an enema consisting of a *strong* solution of nitrate of silver, which was twice or thrice repeated during the

ensuing twenty-four hours. Also camphor spts. and oil of turpentine, equal parts, to be applied almost hot, to the abdomen. It was of no use. The disease increased in severity. We looked upon the mortal issue as being but a few hours in advance of us. Here was our extremity, and *cold water* was the straw caught at. What miraculous buoyancy there was in that dernier resort! We left off medicine entirely—little use was it when none would be retained by the stomach—and determined to try cold water. We wrapped the patient in a cold wet sheet, and thereupon—having previously passed a stool every ten or fifteen minutes—he lay one hour and a half without desire to go to stool. At the end of this time the surface almost glowed with warmth, and there was the moisture of sweat about the face and neck. The patient was then wiped dry with coarse towels and placed in bed. This operation was thenceforward repeated every five or six hours for the next five days, after which time it was only used once or twice in twenty-four hours for two or three days longer. Instead of the warm fomentations, which had been constantly applied, cloths wrung out of cold water was frequently applied to the abdomen. As enemas, we used cold water simply—8 or 10 ounces immediately after every evacuation. In every case in the treatment of which we used cold water injections, it was found to be important that it should be administered immediately subsequent to every stool. They were borne without distress, and much longer. I ought also to mention that, after the first day, we used the Sitz-bath of the Hydropathists. From the commencement of this treatment, the irritability of the stomach was entirely appeased; the stools became less and less in frequency and of a more natural appearance and consistence. As a diet, as well as an auxiliary to the treatment, we ordered the animal broths, well salted.

Several other cases I have in my mind of a like character with the above. With the exception of one, however, none of them were so violently attacked. That case being of a more robust habit, the disease did not succumb so readily. I commenced treating with calomel, ipecac. and *one grain* of morphine every three hours—at the end of twenty-four hours giving a castor oil laxative—warm fomentations to the abdomen; enemas of cold water and laudanum. This course was kept up with more or less modification until the expiration of the week. I was not flattered by the progress my patient

had made for the better. I then resorted to the water treatment—carrying it out as in the first instance. Upon the first using of the wet sheet, the bowels were quieted two hours—having been previously moved as often as from 15 to 30 minutes. The patient kept right on improving—steadily yet, I confess, slowly. It was gratifying to see the complete relief from the excruciating tormina and tenesmus which followed the "wet sheet packing."

In this case I used, as often as once in four hours, the turpentine emulsion, strongly charged with laudanum. I also occasionally ordered laudanum in the injections.

In many cases, the cold, wet bandage and cold water injections were used as auxiliaries to other treatment, and with highly gratifying effect.

I am so thoroughly convinced of the powerful efficacy of cold water in the treatment of dysentery, that I do not hesitate to say I regard it as *one* of the chief remedies for combatting that formidable disease.

Dr. Bennett, of this place, a practitioner of many years standing, and a correct observer, after being repeatedly disappointed by depending upon the ordinary remedies alone, is, upon fair trial, in many instances, enthusiastic in his confidence in cold water as a powerful auxiliary in treating dysentery.

It would be absurd to argue a general rule from such limited experience; yet its effects have been so highly gratifying in the hands of many practitioners, that it is hard to resist the conviction that cold water deserves a more honorable place among the therapia of dysentery than it has hitherto obtained.—*N. W. Med. & Surg. Jour.*

PART THIRD.

FOREIGN INTELLIGENCE.

PRACTICAL MEDICINE.

On Irritable Bladder in Children. By Christopher Fleming, M. D.

Irritable bladder occurs much more frequently in young children than would at first sight appear, and this, where the irritability is

not the consequence either of inflammation or of organic disease, although occasionally attributable to some abnormal defect. The mother or nurse of the child so affected states, that the child is constantly applying the hand to the organs engaged; that it appears to suffer pain during micturition; that the act is frequent; that it is urgent, but when the urine has passed off the child appears relieved; that often, if the urine falls on the floor or clothes, it rapidly becomes muddy and whitish, and it is even stated by some, that it is so at the moment of being passed; that when the child sits down for such purpose, it has an inclination to remain longer than is requisite, and, in in some cases, that there is a disposition to prolapsus of the rectum, from the forcing and straining attendant, and very frequently a discharge of bloody mucus from the rectum takes place; that these symptoms have continued for some time, notwithstanding the exhibition of medicines to regulate the bowels and produce other ordinary effects; that the child is losing strength and wasting in flesh; that the appetite is most precarious, and that there is a great desire for drink; that the quantity of urine passed is very variable, sometimes deficient; that its quality is equally changeable, at times being pale, at others deep in color, and again clear and often muddy, and with copious sediment.

If accurate inquiry is now instituted, it will be found, that many such children are born of gouty parents, or of persons much subject to dyspepsia, and that they are children whose diet and habits of life are irregular; and in the humble walks of life that they are, in addition, irregularly clad and irregularly cleansed: such are by no means of unfrequent occurrence. The quantitative and the qualitative analysis of the urine satisfactorily explains the symptoms; and attention to the physical condition of this secretion, to its chemical constitution, and to the appearance of the deposits—particularly the deposits of rest—assisted by the microscopic characters of the latter, point out the curative indications which are suitable to each variety of case. It is beyond all manner of doubt, as in adult age, many practical hints can be taken from attention to the general constitution of the urine in the surgical diseases of these organs, and that in the child the normal essential constituents of this important fluid may be increased or diminished, and that abnormal ingredients may

be superadded. We have here the lithic, the oxalic, and the phosphatic diatheses, and each has its special influence. In fact, with few exceptions,—and the presence of sugar in the urine of children is one worthy of note,—there is no derangement of the urine found in the adult which I have not also found in the child, in its most exaggerated form, both as regards the disproportion between its normal constituents, and the introduction of abnormal substances. I feel perfectly satisfied that attention to these details, as subsidiary means, will be found of value in the diagnosis and treatment of many of the diseases of children, and especially in those cases of cerebral complication which so repeatedly puzzle the practitioner, and where the quantity of this secretion, on the one hand, is materially diminished, or, on the other, increased.

With a view to the practical study of the morbid condition of the urine, as auxiliary to the diagnosis and treatment of diseases of the urinary organs in the child, I find it difficult to condense my remarks so as to avoid the introduction of any irrelevant matter, and at the same time to escape the charge of an attempt to undervalue those more minute particulars to which, justly, much importance is attached. In my lectures on these subjects, I have been in the habit of directing attention to the color and smell of the particular specimen under examination, to its chemical reaction, and to its density; and I have always attached very great importance to the *deposit rest, as to shade and outline*, and to the transparency or otherwise of the supernatant fluid. From an analysis of these several leading features of urine I have derived the greatest advantage, and even, in the absence of any microscopic examination, have been enabled to decide in very many cases, with sufficient certainty for practical purposes, on the peculiar nature of the deposit. Of course in some, particularly the "non-crystalline organic deposit," the assistance of the microscope is often indispensable; but in children they do not constitute the majority of the cases met with. As attendant on the "irritable bladder," I would say that, according to the classification of urinary deposits by Golding Bird, those of uric acid, and the urate of ammonia, and of oxalate of lime, are particularly frequent in occurrence; and that, next in order of frequency are conjointly or separately with these "the non-crystalline organised products,"

such as blood, pus, occasionally mucus, but very often indeed vibriones. It would be too great an occupation of space and of time to enumerate the many cases I have witnessed, as illustrative of these statements; they are of almost daily occurrence. I do not deny that phosphatic deposits are to be met with, but these deposits do not occur, under ordinary circumstances, as a substantive deposit in the urine of children. The prisms of the neutral triple phosphate are to be seen conjointly with the crystalline deposits above specified, just as in adults, but it is very rare indeed to meet with them as solitary deposits, although so frequent in advanced life; and it is equally rare to find them combined with that physical and chemical condition of urine almost necessarily present under such circumstances. Indeed I find it difficult to bring to my recollection—unless under the most aggravated form of vesical and renal disease, complicated with phosphatic calculus in the child—that excessive secretion from the mucous membrane of the bladder which takes so very prominent a part in the formation of such deposit in advanced life. Amongst the numerous cases of urinary disease I have witnessed in the child, such is excessively rare, and it is equally rare as a symptom of calculus in the child. There was in one case a source of deception, which was by the merest chance unfolded to me, and which, perhaps, may be noted as confirmatory of Sir Benjamin Brodie's opinion as to the special source of those phosphatic salts in the urine.

In May, 1852, a boy, aged three years, was brought to my study by his mother, in great alarm from the suffering the child had endured for some weeks in passing urine. There was frequency and urgency, and so much forcing and straining as to produce distressing prolapsus ani. The urine was largely loaded with lithates, and contained a remarkably tenacious mucous deposit, deeply colored with blood, and adherent to the glass. I found in it numerous blood discs and large crystals of the triple phosphate. Symptoms not improving, I felt justified in sounding the child, which I did with a silver catheter, and whilst the urine was escaping, a severe paroxysm, resembling a fit of the stone, occurred, during which a considerable quantity of gelatinous mucus escaped from the rectum. I collected the urine drawn off through the catheter in one test-glass, and the

discharge from the rectum in the second. The urine was acid, the latter alkaline; the former was loaded with lithates interspersed with some crystals of oxalate of lime, the latter, in addition to mucus, blood globules, and epithelial scales, was studded with large, distinct, triple, phosphatic prisms. I merely mention these details incidentally, as interesting and not unimportant phenomena, and particularly in connection with the statement of Sir Benjamin Brodie already alluded to. They were to me then novel, and I took the opportunity of showing the microscopic appearances and the specimens to my colleague, Dr. Hutton. I have since been enabled to confirm them.

I have said that the lithic acid and lithate of ammonia deposits, and also that of the oxalate of lime, are the most frequently met with in children, and they will be found to be productive of most decidedly distressing urinary symptoms. I have often found these deposits present conjointly; very often the oxalate of lime and the urate of ammonia, the latter cloaking the former, unless carefully looked for. I have found the red sand, as the lithic acid is sometimes termed, in the child, but it is far more frequent to find the colorless, or nearly colorless, crystals of lithic acid, and all are met with of every variety of shape and form, and they are to be seen in the children of the poor as well as of the rich; and really it does not appear that diet very materially influences their presence or their character. I have a boy, aged about seven, now in hospital, with suspected calculus in the bladder, and in him numerous crystals of pale lithic acid exist in combination with oxalate of lime; whilst in another ward in the same hospital, there is a boy, somewhat about the same age, a patient of Dr. Hutton, in whom the red sand is visible to the naked eye in the test-glass, floating through the urine, and under the microscope the deep orange crystals are to be seen distinct, and in large aggregated masses. It is in this class of deposits, and in that of the oxalate of lime, that the surgeon requires to be more circumspect, as the physical and chemical characters of the urine are often not remarkable. The color is in such cases often pale; the density very low, so low as 1007 to 1010; the deposit a mere tomentous, semitransparent cloud, but one which will not escape detection by the practised eye. The suffering experienced in some cases of this nature is really very great; and if the child happens to have a long narrow prepuce, or an abnormal opening in the urethra, he may be put to unnecessary torture from inattention to the

morbid state of the urine. I have known such cases: one, a fine child, the only son of a fond father, who lived freely, and thought the child could not do better; the other, a boy aged three years, with hypospadias, from which the child had not previously experienced any visible inconvenience. In each the usual traces of symptoms of irritable bladder existed; in each, the ordinary clear condition of urine diverted attention from the examination of its actual state; and in each, its normal restoration caused the subsidence of all annoyance. I do not dwell on the lithate of ammonia deposit; its characteristic appearance is too obvious to require any comment. I shall merely add, in conclusion, as regards it and the other crystalline deposits noted, that when from their continued presence they produce local or general irritation, they demand the watchful care of the practitioner to put rigidly in force those dietetic and therapeutic means which are laid down in systematic treatises on the subject; and if the symptoms do not yield to such treatment, he should search for some local cause either in the bladder or in some portion of the organs implicated; and he should also bear in mind, that such character of urine is the most likely to lead to the formation of stone in the bladder, as proved by its composition in the child, and by the fact of its being the most common attendant on its presence when found there; and that hence two practical lessons should not be lost sight of, namely, to alter, as quickly as can be effected, this morbid condition of urine, and to suspect the presence of calculus, should it be obstinate.—*Dublin Quarterly Journal.*

From the Gazetta Medica Toscana, p. 44. Dublin Quarterly Jour., Feb. 1853.

ART. VII.—*On the action of the Subnitrate of Bismuth.* By DR. FILLIPPO LUSSANNA.

The subnitrate of bismuth, a medicine already well known, has been lately used with new indications and pharmacological applications. Notwithstanding the fear which previously existed in its local caustic action, Monneret has recently recommended it in large doses as a topical (internal) anodyne in various gastro-intestinal affections, gastralgia, diarrhœa, dyspepsia and cholerine. He, following the fashion of many of his countrymen, ignored almost every general action of this preparation, and declared that its therapeutic efficacy is purely local, and that, far from being irritating, it is ab-

sorbent and calming. Monneret has rendered much service to science, by destroying, so far as this remedy is concerned, the Orfillian scare-crows which still not a little impede the rational interpretation of pharmacological facts. But, at the same time, I do not believe that all his deductions are correct and true, when he entirely denies the general effects of this metal. Following his directions, I have administered the subnitrate of bismuth in the large doses given by him. The patients so treated were affected with tubercular diarrhœa, the consequence of chronic enteritis, gastralgia of long standing and mesenteric disease. From the study of the effects of this remedy, I have been able to deduce the following inferences,

1. It is true that it does not give rise to intestinal irritation.

2. I have not seen it arrest tubercular or mesenteric diarrhœa.

3. The fæces always acquired a blackish-yellow color, owing to the unassimilated portions of the medicine being converted into sulphuret of bismuth by contact with the sulphureted hydrogen of the fæcal masses.

4. The excrement, although still presenting the character of diarrhœa, was always rendered less liquid in consequence of admixture with the powder.

5. The subnitrate of bismuth is in part assimilable, and some portion of the large quantity swallowed is really dissolved and absorbed. This takes place with many other medicines, of which only a portion can be dissolved or absorbed, while the remainder passes unacted on, when the remedy has been given in large doses, through the intestine. Such is the case with calomel, iron and kermes, among metallic preparations.

6. The assimilation of subnitrate of bismuth, is due to the acid action of the fluids of the stomach, which render it soluble. Any portion that reaches the intestine, whether undissolved or dissolved, cannot be digested or absorbed, because the alkaline chlorides of the intestinal tube have no solvent effect upon it, and precipitate it if dissolved. In this second point of view, Monneret would be quite right in denying the effects of this salt on the economy in general, and in denying its absorption. The subnitrate of bismuth is soluble in acidulous matters, and, consequently, can be digested in the stomach. Thus we can understand how Trousseau and Pidoux, who

were also ignorant of the general action of the salt, should, notwithstanding, caution us against administering it when gastric affections are attended with eructations—in which, say they, "the medicine almost fails," since, by rendering it more soluble and assimilable, the presence of medicine developes its dynamic action.

7. The non-appearance of the subnitrate of bismuth in the urine does not exclude the possibility of its having been introduced into the circulation. In fact, the subnitrate, having entered the current of blood, is, by the action of the alkaline chlorides of the serum, restored to the state of an insoluble subsalt, and thus becomes incapable of passing off by the uriniferous emunctory.

8. The effects of bismuth, thus introduced into the economy, are colliquative and scorbutic. The patient acquires a leaden aspect, the eyes become sunken, and present a livid subpalbebral circle; the breath is rendered offensive, the gums swell, grow livid, and discharge a sanious blood; hemorrhage is easily excited, and sometimes profuse passive hemorrhages arise. I had once under my care a case of dangerous and repeated epistaxis produced by it in mesenteric tuberculosis. Sometimes blood is seen in the expectoration; finally, the alvine dejections are occasionally melanic. Everything announces a solvent action on the globulin, similar to that produced, according to the experiments of Dumas, by the chlorides of potassium, sodium, and ammonium, which dissolve blood corpuscles. It might be classed among Miahle's fluidifying agents.

9. Therefore, when we wish to prevent the absorption, or the true medical action of the subnitrate of bismuth, to localize its action, and produce none but its mechanical absorbent effects, the rational mode will be to precede or accompany its administration by the exhibition of an antacid, as, for example, calcined magnesia, so as to neutralize and fix the solvent acid gastric fluids. And this, of course, will also be the chemical antidote of the preparations of bismuth, with the view of preventing their assimilation, while the preparations of iron and stimulants will be useful in counteracting their general or dynamic effects of producing scurvy—fluidifying and dissolving the hematin.

ART. II.—*Cases of Poisoning with Lucifer Matches.* By C. TRENBERY, Esq., Surgeon of the Civil Hospital, Gibraltar, and corresponding Member of the Surgical Society of Ireland.

Three cases, within a very short period, wherein Lucifer matches were swallowed for the purpose of self-destruction, have presented themselves to my observation. Two of them proved fatal; the third recovered.

The history of these cases may be related in a few words: one, a young Spanish lady, resorted to the expedient from disappointment: the other was a Spanish widow, mother of three children, who, having committed herself with a young man aged 18, hoped, by taking the poisonous substance, to induce him to marry her. The contents of the stomach were dislodged, and she recovered.

The third case was that of a native, aged 22, who was engaged to be married, but unfortunately too close intimacy with her lover prior to this rash act, exhibited itself, she being in the seventh month of pregnancy. Her aunt states that at no time were the symptoms very urgent, nor is it known when the matches were swallowed.

The deceased first complained of pain in the stomach at 5 A. M., on Tuesday the 17th of May, 1853, for which some "Yerba Louisa" tea was given, which relieved her. At 8, A. M., she reluctantly partook of an egg and some tea for breakfast, and at eleven o'clock had some broth but could not bear the sight of bread. During the day she complained of thirst and giddiness: in the evening took tea and went to bed. The next morning, the 18th, about 6 A. M., still suffering from pain and giddiness; her aunt gave her a dose of castor-oil, which she ejected immediately, and frequent vomiting supervened during the day. An enema of the decoction of marsh mallows and olive oil was administered, which afforded temporary relief.

In the evening, she confessed that she had taken two boxes of lucifer matches, containg about one hundred. A medical man was immediately consulted, who ordered an emetic and olive oil, which she said relieved her very much; but vertigo, vomiting, intense thirst, slight convulsions and syncope, followed in rapid succession, and after midnight she became insensible, and expired at 6 A. M., on the 19th. The following day, at noon, I made a post mortem examination, and noted the following: Body well formed, but short of stature. Face has a distorted and dirty appearance from irreg-

ular patches of congestion. Globe of the eyes very prominent; cornea of a dry and dull appearance; pupils dilated, and blood oozing from the nostrils. Livid spots about the lips, gums, and tongue. Upper part of the shoulder quite green, and emphysema from decomposition. Breasts plump, and nipples marked by darkish areola and papillæ. Abdomen distended, and tympanitic, but no appearance of decomposition except in the folds of the groins. Vagina plugged by a dark coagulum of blood; the os uteri dilated to about the size of a shilling, and the head of a child pressed against it, as if labor had commenced prior to death. Dark, feculent, or tarry looking matter escaping from the anus. Finger-nails livid, as also back part of the anus, and depending parts of the sides of the body, and in some parts it is quite green. Lower extremities pale and chlorotic-looking; the calf of the left leg being hard and globular, as if affected with cramp. Gas escaped with a hissing noise from the aperture made by the saw in opening the skull. Dura matter dry and glistening; substance of brain so much softened as to prevent the usual examination. Ventricles very dry. Cerebellum a perfect pulpy, or custard-like mass. The encephalon throughout of a dirty cream color. Pectoral muscles soft and easily torn. Lungs dark colored, but crepitant; their posterior portion congested, as is usually observed after death. Pericardium contained about two ounces of dark bloody serum. Heart pale and flabby: auricles and ventricles empty. Abdominal viscera had a dull appearance. Stomach greatly distended with gas—its external appearance healthy looking. Small intestines collapsed; large intestines not so distended as is usually seen. Anterior surface of liver pale. Gall-bladder empty. Spleen very dark colored. Uterus contained a well formed but dead female fœtus, of about seven or eight months, floating in the liquor amnii, the membranes being unbroken. The cellular tissue and depending or posterior portions of the liver, pancreas, spleen and kidneys, unusually congested, amounting almost to extravasation of blood. The pharyngeal and cardiac end of the œsophagus of a dark or bruised color; the canal appears contracted, and the mucous membrane dry, one rounded, carbonaceous head of a match adhering to its surface. Stomach contained about four ounces of a dark, unctious, treacly-looking fluid. The mucous membrane of a whitey-brown color, and the cardiac or larger end thickly vesicated, as if scalded; the middle portion retained its rugous appearance, although thicker and paler

than usual. The pylorous quite smooth, pale and thickened, as also the mucous membrane of duodenum, which, with about three feet of the jejunum, contained a similar black looking fluid as the stomach. The remaining portion of small intestines were merely lined with a creamy, feculent substance, interspersed with patches of greenish or bilious matter.—*Dublin Med. Press.*

ART. III.—*Further Researches on the Pathology of Phlegmasia dolens.* By ROBERT LEE, M. D., F. R. S., &c.

At a late meeting of the Royal Medical and Chirurgical Society of London, Dr. Lee read a paper on this subject. The author commenced his paper by observing that it was not till the publication of M. Bouillaud, M. Velpeau, and the late Dr. Davis, that the true nature of this disease was known. Up to this period various hypotheses had been advanced respecting the cause of the swelling in the lower extremities of puerperal women—mere speculations unsupported by facts; but the cases and directions of the authors just enumerated, demonstrated that the true nature of the disease consisted in an inflammation of the trunks and principal branches of the veins of the lower extremities. In papers by the author, published in the fifteenth volume of the *Transactions*, the actual condition of the iliac and femoral veins was ascertained, and he had been led to infer that inflammation of the veins gave rise to all the phenomena in puerperal women of phlegmasia dolens, and that it commenced in the uterine branches of the hypogastric veins, and subsequently extended from them into the iliac and femoral trunks of the affected side. Other cases had been recorded in the *Transactions*, of crural phlebitic following ulceration of the mucous membrane of the intestines. Experiments performed by Pirigott in 1539, and by Reumert in 1860, on dogs, showed that the action of the chemical and mechanical irritants was limited to the vein on which the experiment was made, and the extension of the inflammation in the veins was not common; and Stanius, who had collated and tested all the facts bearing on the subject, doubted whether inflammation of venous trunks admitted of being excited by constitutional causes, independently of local irritation. A series of experiments on the veins of the lower animals, similar to those just mentioned, had recently been made, and a paper on phlegmasia dolens had been read to the

Society during the present session, not founded on actual observation of the disease as it occurs in the human subject, but upon experiments on the veins of the lower animals, in which phlegmasia dolens had never been observed. The object of the present communication was to submit to the Society the observations which the author had made during the last twenty-four years in inflammation of the crural veins. The paper contained the record of forty-three cases of phlegmasia dolens. The first nine cases were accompanied by post-mortem descriptions, and preparations illustrating the disease; and the author was led, from the whole of the facts thus adduced, to the conclusions he had formerly expressed, "that inflammation of the iliac and femoral veins gave rise to all the phenomena of phlegmasia dolens, and that the inflammation commenced in the uterine branches of the hypogastric veins, and from them extended to the iliac and femoral trunks of the affected side." The next series comprised the history of twenty cases, which the author thought furnished additional evidence in favor of this conclusion, though, in consequence of the recovery of the greater number of the patients, an opportunity was not afforded of determining by dissection the actual condition of the crural veins. Nine cases followed, which demonstrated that phlegmasia dolens might occur wholly unconnected with pregnancy and parturition, and that in such cases the inflammation likewise commenced in the uterine branches of the hypogastric veins, and followed a course similar to what occurred in puerperal cases. In some of these the inflammation of the uterine veins was produced by cancerous disease of the os and cervix uteri; in others there was no organic disease of any kind previously existing. The concluding cases were five, in which crural phlebitis had followed inflammation of the saphena veins, and of the deep veins of the lower extremities from fracture of the tibia and fibula, and the pressure of encephaloid tumors on the thoracic viscera. The author thought that these cases and dissections, as well as those of the distinguished authors whom he had quoted, proved in the most conclusive manner that inflammation of the iliac and femoral veins was the proximate cause of phlegmasia dolens; and that in puerperal women this inflammation commenced in the uterine branches of the hypogastric veins. It had likewise been demonstrated by morbid anatomy that phlegmasia dolens was a disease which might take place in women who had never been pregnant, and even in the

male sex, and that, under all circumstances, the proximate cause was the same.

Dr. Mayo inquired if, in the cases of phlegmasia dolens recorded by Dr. Lee, any peculiarity antecedent to the inflammation of the veins had been noticed?

Mr. Streeter asked if Dr. Lee had statistical information to give respecting the comparative frequency of the disease?

Dr. John Clark enquired if phlegmasia dolens, in the common acceptation of the term, and independent of other disorders, was always the result of inflammation of the veins. He thought the disease generally was very mild, and, in the experience of writers on the subject, not a fatal one.

Dr. Mackenzie said, that in the discussion of such questions as that which is now before the Society, it appeared to be important to distinguish between the facts which are alleged and the conclusions which are drawn from them. Now, in the present case, the facts alleged are, that certain lesions of the crural veins are developed in the progress of phlegmasia dolens. The conclusions are that such lesions constitute the essence or proximate cause of the disease. He (Dr. Mackenzie) assented fully and entirely to the first of these propositions, whilst he dissented as fully and entirely from the latter; and, as he was unwilling to enter upon the discussion of this question in a controversial spirit, he would make no reference whatever to the investigation which he had lately submitted to the Society on the subject of this disease; but would confine himself to a statement of such facts as were known to the profession, which appeared to him to be opposed to the theory of the disease which had been affirmed by Dr. Lee. The disease known as phlegmasia dolens was a very complex malady. It was one which was characterized not only by a morbid condition of the veins, but by a morbid condition of the sensory, the motor, the lymphatic, and the secretory organs of the affected extremities also: and, accordingly, in all well-marked cases of the disease, there was exquisite sensibility of the limb, especially in the track of particular nerves, loss of motor power, amounting sometimes to perfect immobility of the extremity, inflammation and obstruction of the lymphatic vessels and glands, and a general, hot, tense and elastic swelling of the limb, not simply arising from œdema, but possessing the character of active exudation from passive effusion. Now, could all these lesions depend upon or be deduced from mere inflammation and obstruction of

the principal vein of the extremity? Were they ordinarily observed in cases of simple uncomplicated phlebitis? Or, if not, was there anything in the anatomical or physiological characters of the veins to justify our deducing *a priori* from it? And if we replied to these questions, as he submitted that we must, in the negative, he should ask whether those who adopt this theory have undertaken any particular investigation for the purpose of determining this point? Or, in other words, have they reproduced the lesion of the veins in a simple, uncomplicated form, and observed such consequences to follow. Now, to these questions we must also reply in the negative, and it must be added that the whole matter rested on assumption. It had been assumed that because the crural veins were found obviously diseased in fatal cases of phlegmasia dolens, such lesions constituted the proximate cause of disease. No further steps had been taken to establish the truth of this doctrine, and that, therefore, had been taken as a matter of assumption, which ought to have been made a matter of demonstration. Further, he would observe, that the clinical history of the disease and the progress of symptoms did not support this theory. It was quite true that in some cases the first irritations commenced in the region of the femoral vessels, but in others it was far otherwise; insome they commenced in the back, in others in the hip, sometimes in the calf of the leg, and more frequently in the popliteal region. A gain, one leg might be affected alone, or both concurrently; or the disease, after having attacked one, may pass on to the other, or a superior extremity might be affected; and he had lately met with a case in which, after symptoms of the disease had successively declared themselves in the left lower and upper extremities, the malady ultimately established itself in the right arm, the whole right upper extremity being hot, swollen, and tense, the surface exquisitely painful, with loss of motor power, and tense, corded condition of the basilc vein. Now, it appeared to him that these facts were inconsistent with the theory that the proximate cause of the disease was essentially inflammation of the crural veins. They pointed to the existence of some more general and diffusive cause, in regard to which it was probable that phlebitis itself was but a secondary affection. Again, he would point to the general experience of the profession as being opposed to this theory. It was now upwards of thirty years since it was first promulgated by his friend and teacher, the late Dr. David Davis; and although the facts upon which it rested were well known, it was very far from

being generally adopted. Thus, in this country, Dr. Burns affirmed that the nerves were as much affected as the veins. Others regarded the lymphatic vessels as being principally affected; whilst many, dissatisfied with these restricted views of the pathology of the disease, preferred the theory of the late Dr. Hull, that it consisted in general inflammation of the several organs and structures of the affected limb. So again, on the continent, the greatest difference of opinion existed respecting its nature and pathology; and whilst many affirmed that it consisted essentially in inflammation of the lymphatics, and others that it was a specific inflammation of the cellular tissue, nearly all agreed that in its general characters it differed widely from ordinary phlebitis. Now, this diversity of opinion existed, notwithstanding that all were aware of the facts upon which the phlebitic theory of the disease rested; and it afforded a powerful argument against it, because it tended to show that when tested by general experience, and considered irrespectively of particular facts, and free from bias, it failed to account rationally for all the known phenomena of the disease, and consequently, could be regarded as its proximate cause. Then, in the sequela of the disease, circumstances are met which are inconsistent with this theory. We know, for instance, that after an attack of the disease, the crural veins were generally left impervious or obliterated, and yet it would happen that successive attacks of the disease might occur in the same extremity. Now, if it was true that the first attack left them in the condition described, it was difficult to understand how, having functionally ceased to exist, they could again take on functional activity, and become the seat of active inflammation. So also it happened, after an attack of the dissease, that the limb would be left many years, or even for the remainder of life, in a weak, sensitive and irritable condition, being easily affected by atmospheric and constitutional influences. It was easy to reconcile these facts within the notion that the nerves had been injured or damaged by the attack, but not with the idea that the veins alone had been affected. On all these grounds, then, it appeared to him (Dr. McKenzie) that the phlebitic theory of the disease was either defective or erroneous. But assuming for a moment that it was correct, he would yet observe that it left much which was still to be explained. We had yet to learn the nature of that peculiar inflammation of the veins which was so exceptional and so different from ordinary phlebitis. Did it

depend upon some peculiar disposition on the part of the venous coats to take on diffusive inflammation, or did it depend primarily on the blood? If we adopted the first of these theories, we were bound to state the nature of the pecularity, and the laws of its development, For, to be satisfied with merely giving it a name and to speak of it as a "specific" inflammation, was not to advance our scientific knowledge, but rather to take refuge, or to hide our ignorance under the shadow of a name. If, on the other hand, we accepted the lattar view, and regarded the inflammation as dependent upon some morbid condition of blood, then, indeed, we might reasonably account, not only for the peculiarities it presented, but for all the several lesions of other organs, and the structural changes with which it was associated. Upon this view, also, we might reconcile the conflicting opinions respecting the nature of the disease which had been held by different pathologists, and the variations which it manifested in its symptems and progress, in its different forms. But, in accepting this view, we must forego the theory that phlebitis was the proximate cause of the disease, and regard it, as it really was, as a secondary rather than a primary phenomenon; related to the other lesions of the extremity not so much in the order of cause and effect, but as being like them a parallel effect of some more general and diffusive morbific agent.

Dr. Lee, in his reply, entered into the literary history of the disease under discussion, from the time of Moriceau to the present. He particularly dwelt upon the facts originally published by the late Dr. Davis, and showed at great length his (Dr. Lee's) own labors in this disease. In answer to Dr. Mayo, he said that there were no antecedent symptoms calling for any treatment. He knew nothing of the comparative frequency of the disease. In respect to the views advanced by Dr. McKenzie, he could only say that, whilst he (Dr. Lee) admitted that there might be a blood disease present, it was consecutive to the inflammation in the veins. The inflammation was in reality the proximate cause of the disease. The scalpel had shown this; he thought that was sufficient, and could not understand what further was required.—*London Lancet.*

Art. IV.—*Stomatitis Materni.*

"Has this disease ever existed when the symptoms of *general* disorder were not accompanied by it, traceable to either retention or

suppression of bile?" In the last 15 years I have not found it necessary to take the child from the breast, and my practice has been based on a negative answer to this question. I have witnessed two post obit. examinations of patients, who labored, at time of death, under this disease, in each the gall bladder and duct were clogged with calculi and thickened bile. In the living patients I have observed, in almost every instance, 1st, an anemic condition; 2nd, acidity of stomach, 3d, as a consequence of the last, urinary deposits, *invariably relieved by the free use of alkalies;* 4th, the alvine evacuations either hard or black, denoting the absence of the natural and accustomed stimulus to the bowels, or clay colored, with a tendency to diarrhœa, showing both a want of biliary presence, and the existence of fermentations, which never (I think) occurs when the proper proportion of bile is present.

The presence of anæmia I have not found necessary to the existence of the disease, but if present, it greatly adds to the chances of continuance. Indeed, it often produces the very state of things, on which, I think, the disease depends. In an anæmic condition it is difficult for the system to perform all of its *usual* functions, and at the same time support the new one now set up—the support of the infant, either in embryo or by lactation. For this support a *new* draft is made upon the energies of the system, and this draft is often made upon some *one* organ, instead of being distributed equally on the whole; that one must, of course, lose much of its energy; and, should it be the liver, we have want of its secretion; as a consequence, we have fermentation and acidity of stomach and bowels with its sequents, amongst which is ulceration of certain of the mucous membranes; and, to the existence of this condition of the membranes, neither pregnaney nor lactation are *necessary*, as it will be found under many circumstances in the absence of both these, though *never* under my observation without the previous withdrawal of the accustomed supply of bile. The only difference to be found in the disease under these different circumstances is, that during lactation or pregnancy it is more obstinate in consequence of the draft upon the energies of the system being continuous, and consequently renewing the disease as often as the remedies for its relief are withheld, or until the vital energies are increased by tonics, or otherwise sufficiently to meet the new demand without the special draft.—*Med. Gazette.*

order were not accompanied by it, traceable to either retention or

MATERIA MEDICA.

ART. V.—*Treatment of acute articular Rheumatism by veratrine.* By M. TROUSSEAU.

We have already spoken of this treatment, but consider it of so much importance as to refer to it again. One pill, we have said, containing the tenth of a grain of veratrine, is given the first day, two the second, three the third, increasing the dose one pill each day, and it is rarely necessary to go beyond six or seven. M. Trousseau, knowing the duties of physician and teacher, has not hesitated to experiment with this therapeutical agent before his numerous students; the results have been marvelous. A woman, aged 25 years, entered his wards with a general acute articular rheumatism of three days duration, complicated with endocarditis characterised by a souffle, to the first beat of the heart, extending itself into the calibre of the aorta, and into the arteries of the neck, plainly indicating a lesion of the aortic orifice. It was impossible to find a case more favorable for the therapeutic experiment on account of the extent and intensity of the complications. M. Trousseau gave a pill on Saturday; on Sunday there was already an amelioration of the symptoms, and he gave two pills. Monday, the fever, the inflammatory engorgement of the hands, &c., had almost disappeared: three pills were given. Tuesday, the day upon which M. Trousseau lectured upon the case, the health was perfect, except the condition of the heart. "A fact is but a fact," remarked the learned professor, "but I can hardly attribute this result to chance." Neither can we attribute it to chance, because we have seen M. Piedagnel employ this medicine with a success almost constant; and know many cases which prove its efficacy. The innocence, the insipidity, and the low price of veratrine, will make it, (if farther experience confirms the facts already noticed,) one of the most precious medicines which we possess. We must not pause upon so good a road. What is rheumatism, if not an inflammatory affection, associated with the rheumatismal diathesis? And on the other hand, in a number of cases, what is pleurisy, pneumonia itself, etc., if not inflammatory conditions produced by the same general influence, rheumatism? It is true that the erratic march of the inflammation establishes a sensible difference between acute articular rheumatism, and pleurisy, and pneumonia, but if the principle is the same is there not a reason

for trying the same remedy. We reason with reference to the specific antirheumatismal power which veratrine seems to possess. But if this is not satisfactory it may be regarded in another sense, viz: as a powerful antiphlogistic or non-stimulant, and as such veratrine would be indicated in a general manner against inflammation, since it has been seen in the space of three days, in the dose of six-tenths of a grain altogether, to reduce the pulse. Perhaps we are indulging in too great anticipations with regard to the results of experiments with veratrine, and it will be necessary, at least at first, to limit its application to inflammatory affections, allied to rheumatism; among which we include pleurisy, pneumonia and neuralgia. The aortic souffle in M. Trousseau's patient, but this is often the case with any treatment, and M. Piedagnel asserts that there is a notable diminution in the diseases of the heart consequent upon acute articular rheumatism when it is treated with this medicine.—*Gazette des Hopitaux.*

Art. VI.—*Inhalation of Chloroform in Pneumonia.*

The late journals of Germany publish more than 200 cases of pneumonia treated by inhalations of chloroform. Far from being contraindicated in pulmonary phlegmasia, as had been thought up to the present time, chloroform on the contrary would seem, according to these facts, to modify favorably the inflammatory process of the lung. From among the observations published, out of 193 cases treated by Drs. Wachner, Baumgartner and Schmit, only nine died. Of twenty-three cases reported by Dr. Wawentrapp, of Frankfort, nineteen were treated exclusively by chloroform, and only one died. Every two or three hours the patient is made to inhale th, vapour from fifty drops of chloroform, during ten or fifteen minutese so as never to let the effects reach to a loss of consciousness. All the patients were of adult age, and the disease upon an average had reached the fifth day. In every case it was observed that the chloroform had a diaphoretic effect, which was sometimes produced by the first inhalation, and never failed to manifest itself on the third or fourth day. It gradually diminished the local pain, and caused it to disappear; it calmed the thoracic anxiety, brought back the respiration to its normal type, always appeased the cough, facilitated the expectoration in rendering it less abundant; and, lastly, it reduced the febrile reaction and induced a refreshing sleep three or four days after the inhalations were commenced.—*Gazette des Hopitaux.*

SURGERY.

ART. VII.— *On the Use of Collodion for erections accompanying Blennorrhagia.* By Dr. DORRINGER.

In the *Med. Central Zeitnng*, there is reported a case of a rather curious application of collodion for gonorhœal erections, and the result was such as we would like to see borne out by other cases: A young man, aged 28, was attacked for the third time with a blennorhagia, which was accompanied with such severe and painful erections that the patient could hardly stay in bed for half an hour. After having tried, without avail, both campor and narcotics, Dr. Doringer ordered cold fomentations, and when the penis had assumed its natural size, the application over its whole extent, even including its prostratic portion, to a strong coating of collodion. This had the desired effect, for from that moment the patient had no erection, and suffered only from a slight scalding in passing urine. What proves that the amelioration was really due to the means employed is, that on the morrow, the collodion being taken off, the erections returned, but not so severely, and again ceased on the application of a fresh coating of collodion.—*Med. Times and Gaz.*, from *Rev. Med. Chir. de Paris.*

PART FOURTH.

BIBLIOGRAPHICAL NOTICES AND REVIEWS.

1. *The Action of Medicines in the System, or "on the mode in which Therapeutic Agents introduced into the Stomach, produce their peculiar effect on the Animal Economy." Being the Prize Essay to which the Medical Society of London awarded the Fothergillian Gold Medal for MDCCCLII.* By FREDERICK WILLIAM HEADLAND, M. R. C. S., etc., 8vo. pp. 560. Philadelphia, Lindsay & Blakiston, 1853.

days after the inhalations were commenced.—*Gazette des Hopitaux.*

We often boast of the rapid strides we are making in professional discoveries and improvements, and we certainly have cause to congratulate ourselves and the world upon the fact that our progress is equal to that in any of the arts or sciences—but we have to acknowledge with much regret that our knowledge of the *modus operandi* of medicines, even in this enlightened day, is not much greater than it was centuries ago. But little advance has been made in this department of medical science, while we acknowledge that the importance of this subject is equal at least to any other, we must hail, with considerable enthusiasm, the announcement of any work which promises to illuminate to any considerable extent this benighted subject.

The fact that Dr. Headland has secured the Fothergillian gold medal from the London Medical Society, is almost a guaranty that his work will produce an era in the history of Therapeutics. We have read a portion of it, and from its examination thus far, we are remarkably pleased with the author's plan and his style. We take great pleasure in recommending the work to the favorable consideration our readers.

For sale by J. H. RILEY & Co.

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2. *An Analysis of Physiology, being a Condensed View of the Most Important Facts and Doctrines, Designed Especially for the use of Students.* By JOHN J. REESE, M. D. Lecturer on Materia Medica, In the Medical Institute of Philadelphia, etc., etc., Second Edition, Revised and Enlarged—12mo. pp. 357. Philadelphia, Lindsly & Blakiston, 1853.

This little volume seems to be a tolerably accurate *resume* of Physiology. It was compiled by a young Philadelphian expressly for the use of Students who are probably preparing for the *green room.* For securing an outline of the grand science of which it treats, it is very well adapted to the purpose. It is a kind of epitome of Carpenter, which stands almost alone as an English work on Physiology. It is worthy the consideration of the student who has not time to study larger volume.

3.—*On the Diseases of the Liver.* By GEORGE BUDD, M. D., F. R. S., Professor of Medicine in King's College, London, etc. Second American, from the last improved London Edition, with Colored Plates and Wood Cuts, 8 vo. pp 468. Blanchard & Lee, Philadelphia, 1853.

We take great pleasure in endorsing the very favorable notices of this work by the various Journals of our country. The first edition was well received, and it is now too well known to require an extended notice at our hands. Although a work of great merit on a subject of the highest importance, particularly in our western country, which is full of Hepatic diseases it has not found an extensive sale. Physicians generally poor, especially in the early portion of their professional career, are almost obliged to content themselves with standard works on the general subjects of medicine and surgery. The *Monographs* have to be neglected until a thirst for more extended knowledge and pecuniary ability demand their purchase. As these do not always make their appearance in the person of *every* member of the profession the Monographs do not find a place in *every* medical library.

We can speak in the highest terms of "Budd on the Liver," and commend it to the careful perusal of every physician in the west.

For salè by J. H. Riley & Co.

4.—*Medical Jurisprudence.* By ALFRED S. TAYLOR, M. D., F. R. S., &c., &c. Third American, from the fourth London edition. Edited with additions by Edward Hartshorne, M. D., one of the Surgeons of Wills' Hospital, etc., etc., 8 vo. pp 621. Blanchard & Lee, Philadelphia, 1853.

A Treatise on the Medical Jurisprudence of Insanity. By I. RAY, M. D. Third edition, with additions, 8 vo. pp 520. Little, Brown & Co., Boston, 1853.

It is said that physicians never appear to so great disadvantage as when they are giving testimony before a jury. Were this not a fact it could scarcely as it has, become a proverb. Their ignorance, awkwardness, and disgraceful contradictions, are doubtless the result of entire neglect of Medical Jurisprudence as a subject of thorough study. The standing of our profession is wounded and

injured more in the estimation of the legal profession, and the world, in our courts of justice than in any other way. When a physician is summoned to testify in a case involving professional knowledge, if there are any to be found, he directly resorts to a work on Medical Jurisprudence, and in the course of a few minutes, with a mind entirely unprepared by experience or previous education, more than that of a ploughman or mechanic would be under similar circumstances, he rushes before the scrutinizing court with the utmost importance but to disgrace his calling, and to be torn to pieces and made ridiculous by a set of merciless lawyers. While this may not be the universal fact, the exceptions are altogether too few and too far between. There is no necessity for the existence of this state of things, and for a physician, with the means of education now in our possession, to be ignorant of Judicial Medicine, is entirely inexcusable.

The works whose title pages are at the head of this notice, are the very best in our country. Taylor has for a long time been the standard work on Medical Jurisprudence in Great Britain as also in this country. Ray is an able writer, and has left little or nothing further to be said on the subject of the Jurisprudence of Insanity. His chapter on "the duties of the Medical witness," is said by eminent judges to be worth the price of both volumes. They are gotten up in the best style of their respective enterprising publishers, and reflect the highest credit upon them and their learned authors.

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PART FIFTH.

EDITORIAL AND MISCELLANY.

Volume Sixth.

As the present number commences the sixth volume of "The Ohio Medical and Surgical Journal," we take the opportunity to offer its patrons and readers our grateful acknowledgements for their patronage and support. Indeed, we should deem ourself *ungrateful* were we to withhold such an expression. Our Journal is now entirely beyond embarrassment, and will continue to be so if its friends meet

promptly their indebtedness. We ask them most respectfully to do so. We have been cheered not only by the constant enlargement of our subscription list but by the frequent expressions of satisfaction on the part of many of our most intelligent subscribers with the character of the Journal. At the commencement of our editorial labors we had no expectation of meeting the demands of our readers and rendering such general satisfaction. *Then* our intentions were as pure as now but we lacked experience. In the future we shall be more familiar with our duties, and shall endeavor to make our Journal a practical one—an exponent of the orthodox doctrines and sound practice of our profession. Ours is a *Western Journal*, and we expect to make it worthy the patronage of Western physicians.

New Subscribers.

We take the liberty to address copies of this number to many physicians who are not as yet subscribers to our Journal. We do this because we cannot employ an agent, and because we know there are many physicians in our country who would be glad to subscribe for it were they acquainted with its merits. This we would not say boastingly, we but speak the sentiments of hundreds who have written to us on the subject. If, on careful examination, you, gentlemen, who have not had an opportunity to examine it before, find it deserves your consideration and support, be kind enough to send us the subscription price, two dollars, at your earliest convenience. If for *any* reason you are not disposed to become subscribers we beg you to return this number to our address with your name and Post Office address appended.

Original Matter.

Most of the original articles contained in this number are unusually interesting, and it is to be hoped they will not be passed over without a careful perusal. For many months the health of the Editor has been so infirm that he has not been able to write original papers for the pages of his Journal. This he regrets as he has many rare and interesting cases which he is anxious to report for the edification of his friends. He is sometimes, from severe indisposition, compelled to contemplate a period to his labors not far distant. The

thought that in the midst of his labors, and he trusts to some extent of usefulness, his communion with his professional brethren and co-operation with them in every good word and work must find an abrupt termination, is painful in the extreme. Professional brethren are constantly and successively falling at their post. Few reach a ripe old age in consequence of the arduousness of their cares and toils, and why should he expect to live forever? May the writer and reader so live and act that we may hear the echos of the cheering plaudit of "well done good and faithful servant."

Howard's Infirmary.—Perhaps it may not be improper to say to our friends that our Infirmary, designed for the treatment of Chronic diseases of all kinds, particularly for those of a Surgical character, is in a prosperous condition. It is fully organized, and has been in successful operation for nearly one year. The advantages which this Institution possesses facilitate the treatment and expedite the cure of many very obstinate cases which can scarcely be managed in private practice. We invite the attention of physicians to it, and shall be flattered should they commend suitable cases to our care.

Professor Hamilton's Fracture Tables.—We take pleasure in calling the attention of the profession to the fracture tables of Professor Hamilton. Although published in an unassuming form they are a rare and choice contribution to our Surgical statistics. In the following letter addreesed to Dr. Reese, editor of the New York Medical Gazette, the reader will see the author's scope and design

Buffalo, July 20, 1853.

"My dear Doctor—I send you by this mail a copy of the second edition of my "Fracture Tables," just published. They are much enlarged by the addition of a supplement.

"Everybody is looking just now to find some means of arresting prosecutions against surgeons for alleged mal-practice. Dr. Crosby seeks to accomplish this by refusing to undertake the treatment of any case of fracture until the patient or his guardian has signed a bond that he will not prosecute. Several eminent jurists inform me that such a bond is of no value or force in this State—that a surgeon cannot be released from his responsibilities. The Massachusetts

led to contemplate a period to his labors not far distant. The

Medical Society have made a report upon the subject. Dr. Gross, of Louisville, writes me that the mania for prosecutions has reached Kentucky. Indeed I have received communications from various sources in more than one-half the States in the Union which constantly and uniformly speak of the frequency of these suits, and of their unjustness. I have thus far escaped, and so also have one or two other surgeons of Western New York; but it is safe to say that not more than one out of every ten who practice Surgery to any considerable extent, but have been compelled to defend suits of this kind.

It seems to me that the cause of all this is to be found in the fact that surgeons themselves have believed, and taught, and testified, that in a large majority of cases, broken limbs may be made perfect, while the fact is not so! On the contrary, in a very large majority of cases the limbs have been and will, I fear, of necessity, continue to be left imperfect after fractures. This is what my Tables have at length *established*. In them you see not the practice of empirics, but the practice only of the best surgeons. In not a single instance has a case been admitted which was treated by an irregular. Many of the cases are my own. Many of them were treated in the New York City hospitals; some in foreign hospitals. In the examination of these cases I have used great care. I have handled and measured them all myself. The measurements were made always in the presence of medical gentlemen, and I have noted carefully every circumstance, even to the name and standing of the surgeon, and also the treatment. In the tables, however, the names of the surgeons has for reasons which you will readily understand, been always omitted.

I believe I have had no selfish motive in the labor which I have thus accomplished. It has cost me much time and some money, but in this cause I have long been accustomed to spend both freely. I have testified for my brethren in nearly every county in Western New York once or more, and I have given my testimony under commission nearly as many times more for every state around me. I believe, therefore, no one will charge me with a wish to speculate in offering this book; yet I say frankly I hope it will be sold, not for my own sake, but for my fellow's sake. Yours sincerely.

FRANK H. HAMILTON."

The profession is indebted to our friend Dr. Hamilton for his patience and industry in getting up these tables. They will be of in-

valuable service to those who are for sinister purposes prosecuted for mal-practice in bone Surgery.

The price is 37½ cents for a single copy, or 25 cents each for four or more copies—to be had of Phinney & Co., of Buffalo.

Professional Changes.

The present year has been marked by very numerous changes in the faculties of Medical Colleges all over the country, several of which we have heretofore chronicled. By the announcement of the New York Medical College in another column, it will be seen that Professor Whitaker has resigned the chair of Anatomy; Professor Peaslee has been transferred to that department; and Dr. E. H. Parker, of Concord, editor of the N. H. Journal of Medicine, has been appointed to the chair of Physiology, &c., in this school. These are both good appointments, and ought to strengthen the claims of the Faculty on the profession and the public.

In the Ohio Medical College, Professors Locke, Baxley, Cobb and Rives, having resigned, the Faculty was reorganized by the election of Dr. G. W. Bayless to the chair of Anatomy; Dr. Asbury Evans to the chair of Surgery; Dr. N. T. Marshall to the chair of Obstetrics; Dr. S. G. Armor to the chair of Pathology and Physiology; Dr. Charles W. Wright to the chair of Chemistry, and Dr. Thomas Wood to the chair of Surgical and Practical Anatomy. Dr. H. E. Foote has been appointed to the chair of Chemistry in the Miami Medical College of Cincinnati.

EDITOR'S CORRESPONDENCE.

Dr. Howard: I submit, for publication in the Ohio Medical and Surgical Journal, the following condensed report of the testimony, so far as it relates to legal medicine, given in a case the trial of which has just concluded in the District Court now in session in this place, Judge Belden presiding.

The defendant, Geo. P. Smith, a man of mature years, but below the medium stature, was charged, in a bill of indictment, with the crime of rape, or an assault with intent to commit rape, upon the person of Sarah H. Walker, aged eleven years and some months,

on the 8th day of May last; and the following testimony was introduced to establish the truth of the allegation.

The prosecuting witness, Sarah, being sworn, states, that on the 8th day of last May, about 4 o'clock P. M., she was sent, in company with her younger sister Louisa, aged 10 years, to No. 13, in the Tremont House, distant about three or four minutes walk from her residence in the town of Massillon, O., with a handkerchief belonging to said Smith, which her mother had previously washed and ironed—that on entering said room, they delivered the handkerchief to Smith, whom they found alone—that he asked them to sit down, and enquired if they would not like to be mesmerized—that they replied that they did not know any thing about it; thereupon he gave each a piece of metal to hold in the hand, and locking the door, directed them to look steadily at the objects they held until their eyes grew dim, when they would go to sleep—that they did so look for a few minutes, when they went to sleep—that Smith then took Sarah, the elder, up from her chair, led her to and laid her on the bed, raised her clothing from the lower part of her person and laid it on her breast—that he then unbuttoned his pantaloons, exposed his private parts and consummated the act of Congress by the actual entry of her private parts—that before he entered her, he raised her legs, and separated them so as to put them one on each side of his body—that he remained about five minutes in this position—that he then withdrew from her and took her from the bed, tried some trifling experiments in mesmerising with her and her sister—then unlocked the door and let them go—that they went directly home, where they found their father, little brothers and mother, to the last of whom she (Sarah) disclosed what the defendant Smith had done to her, and complained that her drawers were wet, and also that she was sore—that her mother examined her clothing, took off her drawers and hung them on a line, then went out and called Mrs. Ross to come and see them, and that she and Mrs. Ross went out and came back with Mr. Terry and Mr. Keith—that before very long, Dr. Hurxthal and Dr. Schutzer came and examined her and the drawers she had on when she was in the room with the defendant. On being cross-examined, she stated that her eyes were closed during all the time she was in the bed—that she could have resisted if she chose—

that she was not under the control of his will—that she could have taken her limbs from the position in which he placed them, or made outcry; but that she did not know what to do. She further stated that she remained in the room about ten minutes after she came from the bed—that she laughed while there, on Smith's telling her it was not her sister she was sitting on the bed with when her eyes were opened, &c. &c.

Margaret Walker, the mother of the girls Sarah and Louisa, being sworn, corroborates the former as to her being sent to the Tremont house, No. 13, in company with her sister, at the time specied, by her; and states that they were gone about twenty minutes from home when they returned, and Sarah related the circumstances precisely as they are detailed in the preceding testimony before the jury—that she examined the clothing and person of Sarah—that the drawers were wet underneath the private parts, and the latter swelled and red—that the drawers were taken off and hung on a line—that in a few minutes several individuals were made acquainted with the facts by personal inspection, and that amongst these were Drs. Hurxthal and Schutzer. She further states that the girl was sick for a week subsequent to this time; complained of pain on making water—that it felt like a knife cutting her; and had pain in the back.

Louisa Walker corroborates her sister Sarah in all the particulars up to the time of the eyes of both being closed—heard him unbutton his pantaloons (as did her sister,) "heard the bed *squeak*," &c.

Mrs. Ross corroborates the mother as to the drawers being wet, and complaints of the girl Sarah.

Dr. F. T. Hurxthal, being sworn, testifies that about dark on the evening of the 8th day of May last, he was called upon to go to the house of Mr. Walker, and make examination in reference to an alleged assault upon Sarah Walker—that he went there accompanied by Dr. J. Schutzer.

That on their arrival, they were requested to examine a pair of drawers which were presented and had the appearance of such as would be worn by a girl of Sarah's size and age. Found on the lower and back part of the seat of this article a spot of considerable size, still very damp, and which yielded the odor of semen. The

stain on the drawers is such as would be left by semen. We made the examination by candle-light. Dr. Schutzer laid the girl down on a bed—raised and separated the legs; and I examined the private parts of the girl. Saw no marks of violence—no blood or evacuation, and no swelling. On separating the labia, discovered on the left internal surface, increased vascularity, as I thought; that is, I regarded the color a deeper red than natural—a purplish red—attempted the introduction of the little finger, but the complaint of pain was so great that I desisted—did not enter the finger more than half an inch. Subjected the spot on the drawers to no other examination or test than that of smell, and appearance to the eye by candle-light. Can say with certainty that the condition of the drawers indicated semen. Should regard it impossible for an adult to have entered the girl. Semen has a peculiar odor—cannot say what this odor is.

Dr. Schutzer. I examined the girl with Dr. Hurxthal—saw nothing unusual—nothing wrong. There may have been increased redness. She complained of no external injury. Examined the drawers—saw a stain on one side—it was not perfectly dry to my sense of feeling. I thought it had the odor of semen. If this little girl had never menstruated, could not have made the mark or the odor. I have since thought we did not do the subject justice in our examination. If the hymen had been ruptured, there would have been some evidence of it.

On the defence was called out the following:

Dr. L. M. Whiting being sworn, says, that he regards the microscope as the only sure test for fluids suspected of being seminal, and that by the exhibition of spematozoa; that in the absence of these animalculæ, we cannot declare with certainty that any fluid or discharge is seminal—that *odor* is not a test of semen—that there are certain chemical manipulations which might render the existence of semen very probable; but in the present state of science, and where grave consequences depend upon the establishment of this fact, the actual presence of spermatozoa should be insisted upon—that girls of all ages are liable to discharges from the urino-genital apparatus, varying in color, consistence and odor, with the variations in pathology of the parts concerned in their production—that the spot upon the drawers now before the court, for aught he can see, might be

produced by many other agencies than the deposition of semen with its associated secretions—that it may be made by seminal discharge. That a man having at every advantage a young female, and making any vigorous efforts to accomplish a design to commit a rape upon her, she making no resistance, could hardly leave her person without some indubitable evidence of violence—that girls of tender years are tenderly disposed upon being subjected to examinations per vaginam, and complaint of pain is an almost constant concomitant. Cannot state positively how long an ordinary seminal emission would remain wet—this would depend on the condition of the atmosphere, and the perfection of exposure—from half an hour to an hour should think time enough to desicate it when hung on a line and spread on muslin. Should think evidence of injury to person from violence of the sort here contemplated, as contusion, &c., would be more likely to exhibit itself some hours subsequent to its infliction than immediately after.

Dr. T. C. Shreve, sworn, says he considers the microscope the only sure test of seminal discharge—that this is the doctrine of the authorities: quoted Orfila to the effect, that the saliva, under certain circumstances gives off "the true spermatic odor:"—that he does not regard the sense of smell as a reliable means of detecting semen—thinks the odor which is spoken of as *peculiar* is transient, and the evidence presented by it would disappear in a short time—that the seminal discharge dries rapidly when exposed to the air—that the appliance of force to any great extent, by a man standing in such relations to a female child as have been here described, for the purpose of accomplishing an intent to commit a rape upon her, must have been followed by the evidence of personal injury on the female.

Drs. H. M. McAbee, A. W. Whiting, and L. Lamborn were called—all of whom corroborated the testimony of the two last named medical witnesses.

Mrs. Walker was recalled, and testified that her daughter Sarah had never menstruated, and never before or since the day of her visiting the defendants room have her clothes exhibited any such appearance as that upon her drawers now in court—that she has ever been well.

Dr. L. M. Whiting was recalled and enquired of as to effects of mesmerism. In reply, he states that although he has seen many experiments, and read much in relation to the influence exerted in this agency, he does not yet understand well enough the laws of its action to speak definitely in regard to it: but, from his own observation, and reading what may be called authorities on the subject, he would consider the accomplishment of perfect controls over two subjects at the first sitting, in the space of a very few minutes, as very remarkable. Cannot say what time is requisite for a mesmerizer to get control of the will of a subject.

Remarks.—This case presents several prominent points of interest, amongst which are, the youth of the prosecuting witness, the complication of the element of *mesmerism* with it—and the nature of the testimony introduced to corroborate the statements of her upon whom the outrage is claimed to have been perpetrated. A full report of the trial is necessary to show the fact that circumstances exist of a nature to cast at least a shadow of suspicion over the integrity of some of the principal witnesses for the State, and also that the defendant was surrounded by abundant means for the indulgence of passion, without subjecting himself to the commission of a heinous offence against the laws. With the knowledge of these facts, it cannot cease to excite "especial wonder" that a man having a wife and children, and intelligence, should seriously attempt the gratification of inordinate desire upon a child: and that wonder should naturally grow into astonishment when we learn that, entertaining this purpose with every facility for its accomplishment, he should leave no decided traces of violence upon the person of the assaulted. Again, it becomes a matter of great moment to know how far the courts will go in receiving the testimony of persons who, by their own confessions, are, at the time of occurrences in regard to which they testify, in an unnatural state;—that is to say: Will our courts admit testimony going to establish important facts, when the knowledge of the witness is obtained while the functions of organs, and appratuses by the use of which such knowledge is usually obtained, are either partially or entirely suspended?

In relation to the corroborative testimony in this case, it would seem that, without the presence of the "*spot*" upon the clothing—

the story of the girl would stand entirely alone. Hence it becomes a matter of profound gravity to determine as certainly as the nature of the case will admit, what was the material deposited there, and the propriety of calling in the aid of science is apparent. Equally clear does it appear that no equivocal means should be used in an investigation where consequences so important are depending as the reputation, the personal liberty, the *all*, in short, of the citizen. This being the case, it is to be hoped no member of the medical profession will permit, under any circumstances, the escape from his lips of an opinion in such matters, which he has not "the book" to swear by, that it is absolutely correct: for the contrary course *may* tend immensely to prejudice community, and perchance, bring down upon an innocent head the lightnings of public indignation.

L. M. W.

Canton, Aug. 6th, 1853.

August 7th. The jury, after being out during the night and half of this day, have returned a verdict of "guilty of assault with intent to commit a rape."

August 9. On motion, a new trial has been granted by the Judge.

MEDICAL NEWS.

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Medical Examiner

MARIETTA, OHIO, *Aug.* 12, 1853.

Dear Sir—I received upon day before yesterday, the number completing the fifth volume of the Ohio Medical and Surgical Journal.

I have read it with both pleasure and profit. And do hope, and trust, that your valuable Journal will be pecuniarily well sustained. As an evidence of my sincerity in this matter, you will please find two dollars enclosed, to pay my subscription for the next year.

And as I am now writing, please let me trouble you for a few moments with the history of a case that I have now under treatment. I do this hoping that you will answer this by letter, and make me any suggestion in relation to the future treatment of the case that may occur to your mind.

The man was attacked with acute Pleuritis of the left side, on the 20th day of March last. And having been inefficiently treated, the disease continued in a chronic form until it terminated in abscess of the Pleura, and an effusion of pus into its cavity.

This was the condition of the patient, when he put himself under my care. I gave him mercurials with the view of arousing the absorbents; but at the expiration of ten days, respiration became so difficult, and the action of the heart so much impeded, from displacement and compression, that I resorted to the operation of Paracentesis thoracis, which I performed on the 24th day of July upon the plan laid down by Druitt. The operation discharging about twelve pints of a greenish yellow pus; aad during the subsequent week about one additional gallon.

Since which time his side has been discharging upon an average from a half pint to a pint, per day. For a few days after the operation, he had a slight hectic fever, which entirely disappeared under the use of a generous diet, and mercurial alteratives, which have been continued up to within the past three or four days. At which time I substituted the Iodide of Potassium, in conjunction with Sarsaparilla. One of the singular features in the case is the position of the heart, the apex of which, was before the operation, and is still beating in the right mammary region, just below the nipple.

I thought that after the fluid was discharged, the heart would resume its natural position, But this is not the case. Can there be

adhesion between the Pericardium and Mediastinum? Or can it be held in this position by fiberous bands, extending from the anterior to the posterior part of the Pleura. This position does not appear to obstruct the circulation in the least. Does it not form rather a short curve or angle in the Aorta, which we might reasonably expect to impede, and enfeeble the circulation?

May I expect to see my patient recover with the heart in this position? Please answer me by letter, and pour into my limited mind all the light you can upon the case. My patient to all appearance is recovering.

Respectfully,

P. COOKE.

Duty of Medical Men.—Dr. Todd, in his farewell address, on resigning his professorship, made the following remarks:—

"It appears to me, that when a man proposes to devote himself to the practice of an honorable profession, he has a twofold duty to perform; first, to fit himself to the utmost of his ability for the practical duties of that profession, and, secondly, having done so, it is incumbent on him to divest himself, as far as possible, of every engagement which may interfere with his bending his thoughts and attention to the various anxious, difficult, and often perplexing questions which are continually arising in the course of his professional practice. Every member of a liberal profession should keep it constantly in view that he exercises his calling not simply for his own personal benefit, but for the public good, and for the good of his profession at large. So every practising physician or surgeon, whether the sphere of his labor be within wide or narrow limits, should bear in mind that in successful application of his art, by fair, honorable, and truthful means, is involved the repute and estimation in which his profession is held by the public at large. Let each of us act under the feeling, that to himself specially is committed the keeping of the honorable character and the scientific credit of our common profession, and he will have the strongest motives, not only to eschew everything that savors of charlatanical pretence, but to seek for and insure the highest means of moral and intellectual culture.—*Med. Gaz.*